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VOL. XIV NO. 54

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Presidential Address by

PROFESSOR P. M. S. BLACKETT, F.R.S.

AND

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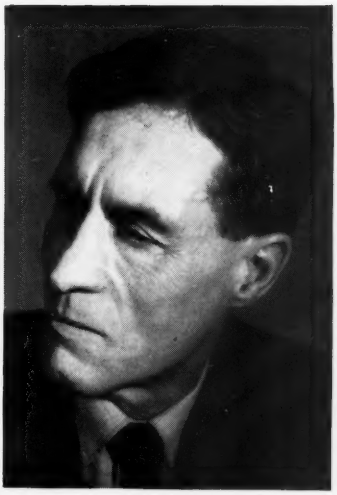

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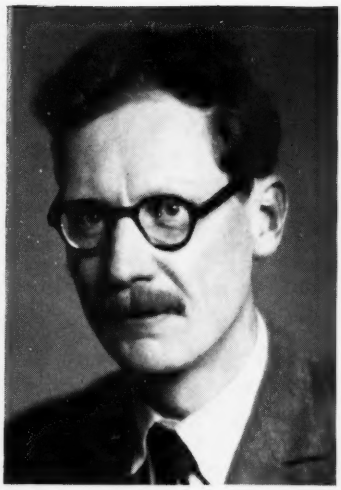

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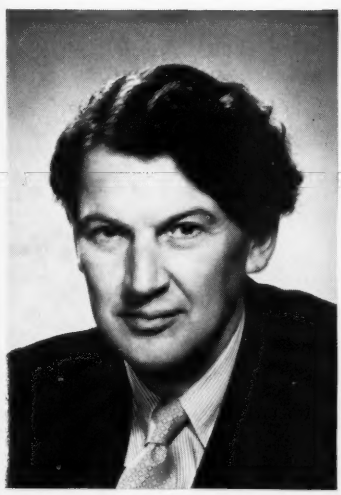

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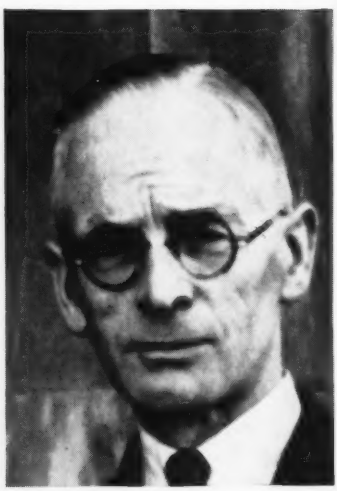

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TECHNOLOGY AND WORLD ADVANCEMENT[1]

ADDRESS BY PROFESSOR P. M. S. BLACKETT, F.R.S.
PRESIDENT OF THE ASSOCIATION

I

SCIENTISTS have often been reproached for not concerning themselves sufficiently with the effects—for good or ill—of their discoveries on society. This charge can hardly be laid against recent Presidents of the British Association. Most of my illustrious predecessors of the last decade have dealt in one way or another with the impact on contemporary society of the vast eruption of scientific and technological discovery, which is by far the most historically significant facet of our epoch.

Last year a special committee was appointed by Council to re-formulate the objectives of the Association in the light of present-day conditions—so different from those of 1831, the year of our founding. One of their recommendations was that still more stress should be laid on the social consequences of modern science and technology. I think that this advice is wise. However, I cannot deny that to follow it is to risk becoming controversial. I do not mean controversial over matters of scientific fact and theory, but controversial in a plain political sense.

For in science and technology lies the key to wealth; and the creation and distribution of wealth are the warp and woof of the many-patterned texture of politics. Many scientists and engineers—let us be quite frank about it—have in the past deliberately avoided thinking more than they can help about the social consequences of their collective achievements: just because, to do so, would sweep them into the troubled waters of political controversy. Some of our scientific predecessors were far less inhibited. It is worth recalling the forthright dictum in 1802 of the young Humphry Davy: 'The unequal division of property and labour, the differences of rank and condition among mankind, are the sources of power in civilised life, its moving causes and even its very soul.' Davy, who has been called the major prophet of the new class of applied scientist, had no scruples about expressing frankly his views on a major problem of his day. I will try to emulate his forthrightness in expressing my views on what seems to me to be the major and very different problem of ours.

It has often been held that the development of natural science and technology gives rise to political controversy only because of the backwardness of the social sciences. If the social consequences of technology, and indeed the problems of society in general, could be studied with the same objective impartiality which has worked such miracles in the physical world, then, so it is alleged, much of the sordid controversy of politics could be avoided.

This is less than half a truth. While all will echo Lord Adrian's plea at Oxford in 1954 for a more rapid development of the social sciences, one must be clear about their limitations. The facts discovered by social scientists—including economists—and the theories which they elaborate, cannot by themselves define the objectives of social policy: what the goals should be must ultimately rest on a judgment of value by individuals. Such judgments of priorities, when made by social groups, become political programmes.

When technological advance increases a nation's power to produce wealth, how should this be used? How much should be devoted to more material goods and how

[1] Address delivered at the Dublin Meeting of the British Association on September 4, 1957.

much to more personal leisure and to gracious living; how much to providing greater consumption now and how much should be set aside for the future; how much to greater opportunity for youth and how much to greater security or luxury for old age; how and to what extent should different kinds of ability and enterprise be rewarded? The social scientist can assist us to think rationally about such alternative uses for our material wealth and may help us to attain any end we choose; he cannot make the choice for us.

II

Though few today in the rich Western world are naïve enough to think that an increase of material wealth necessarily results in a corresponding increase of human happiness and personal fulfilment, yet few would deny the validity of using a nation's material wealth as a measure of its potential welfare.

One of the most striking features of our contemporary world is the very uneven distribution of material wealth. Leaving aside the countries in the Soviet orbit, as needing separate discussion which will not be attempted, we have at the one extreme the highly industrialised countries of Europe and of North America and Australasia, with, in round figures, a population of 400 million, and an average income a head of £300 a year (at 1949 prices). For Europe alone the average is £200 a year. Within this group of relatively rich Western countries there is, of course, a wide range of average income, from one-third to more than double the average. Three hundred years ago, the pre-industrial countries of Europe had a real income a head of not much more than one-tenth of what it is today. In the last half-century their wealth has been increasing at a rough average of nearly 2 per cent a head every year, thus doubling in rather less than forty years. This huge and historically rapid rise in individual wealth was accompanied by a very large rise in population.

In marked contrast to the rich West are the still pre-industrial countries, particularly those of Asia, Africa and South America. These—excluding Soviet China—have a population of some 1,000 million people and an average income of about £20 a year a head, that is, one-tenth that of Europe and a still smaller fraction of that of the West as a whole. Moreover, this low income does not on the average seem to have increased much during the last three centuries and may at times have fallen, despite the revolutionary improvements in world technology as a whole during this period. It seems from such figures that 300 years ago the standard of life in at least the most advanced nations of the East, such as China, India or Persia, must have been as high as that of Europe. The gap now of ten to one in favour of Europe is of quite recent origin and is due to the sudden advance of the West, mainly in the last 200 years, whereas the Eastern countries have remained nearly static.

Economists have recently been turning their attention to the complex historic causes which lead a country to transform itself from a static pre-industrial state to a growing industrial one. Making apt use of an aeronautical metaphor, the American economist Rostow has named this vital period of transition 'the take-off into sustained growth'. In Britain, the first country to become fully industrialised, this critical period seems to have been the last twenty years of the eighteenth century.

In a typical pre-industrial country, three-quarters or more of the population may be engaged in agriculture, and wealth tends to remain constant or rises but slowly. Savings and gross investment are low, some 5 per cent or less of the national income, that is, only about enough to maintain a static economy by paying for the depreciation of existing wealth. After take-off, savings and gross investment rise till some 15 per cent of the national income is available for gross investment, leaving around 10 per cent for net new productive investment. On the average in the West today such new investment results in a rise of total income of about 3 per cent a year. Allowing for the population rise of some 1 per cent, this gives an increase of wealth

a head around 2 per cent a year. The fraction of the population engaged in agriculture steadily falls as social development and industrialisation proceed, and agriculture itself becomes partly industrialised and so much more efficient. In Britain, the savings and investment required for the take-off were essentially provided by the prosperous classes who did not spend their surplus wealth on ostentatious living but invested it in productive industry. It is to this fact which Humphry Davy referred in the remark I have quoted.

Everyone recognises that most scientific and technological innovations can lead to continually increasing wealth only when they become embodied in material things, particularly production goods such as machine tools, chemical and fertiliser plants, transport and communication systems. Nuclear power, the outstanding technical development of this decade, adds to wealth only when nuclear power stations have been built. However, the large amount of capital required is not always appreciated, especially by scientists and inventors, who naturally wish to see their discoveries and inventions used. For heavy basic industries such as steel, oil refining or heavy chemicals, a capital of £5,000 to £10,000 a man employed is required; for medium and light manufacturing industries perhaps from £1,000 to £3,000. Such sums must come from governmental, corporate or personal savings, that is, at the expense of present consumption; or, alternatively, from external loans. The high capital cost of industrialisation is the main reason why the take-off is such a difficult operation for the pre-industrial countries. This is especially so at present because most production goods must be imported, thus making heavy demands on foreign exchange, which, except in a few oil- and mineral-producing countries, is chronically short.

In addition to maintaining its existing wealth, the Western world is saving and investing productively some 10 per cent of its income of £300 a year a head; that is, some £30 a year a head is being invested in additional plant and machinery to create more wealth. The pre-industrial countries of Asia only have about £20 a head to live on, that is, for both consumption and production goods. The West is thus saving more than the East is spending on everything. No wonder that the gap in wealth between the West and Asia is steadily widening. Moreover, most new scientific and technical discoveries or developments tend to widen the gap still more just because the already rich countries have the capital to make full use of them, but the poor countries have not. For instance, under present conditions, the advent of nuclear power must tend to widen the absolute gap still further, because of the high capital expense of nuclear power stations. Decidedly, scientific and technical invention is no fairy wand to wave over a poor country to transform it into a rich one.

III

Anyone who wishes to make himself familiar in detail with the degree of poverty of the underdeveloped countries of the world, particularly those of South-East Asia, India, Pakistan, Ceylon, Burma, Indo-China and Indonesia, will find abundant material at his disposal. In recent years there has been a spate of blue books, white papers, newspaper leaders, speeches, tracts and academic treatises, in which can be found statistics, eloquent writing, moral and political fervour. Of special importance were President Truman's 'Point Four' in his inaugural speech in 1949, and the Colombo Plan in 1950.

When after the Second World War these countries threw off alien rule, the ills of South-East Asia—poverty, underfeeding, disease and illiteracy—became important factors in international relations. These new independent countries became free to make their own external and foreign policy, based on their own views of their own interests. They can now choose for themselves with which countries they will trade or with which they will have close diplomatic or military relations. Moreover, no political party in these new Asian countries can hope to maintain itself long in

power if it does not succeed in improving the lot of its people. The pre-industrial countries of the East can only become less poor by saving and investing money in better education, agriculture, social services and industrialisation. If they are to do this out of their own resources alone, they must inevitably increase their poverty temporarily.

Several careful investigations have been made of the needs for external financial aid in the form of long-term loans and gifts to enable all these have-not countries to make a good start on the road to prosperity. It seems that the *immediate* need is not far from the round figure of £1,000 million a year, that is, £1 a year for each of the 1,000 million inhabitants of these countries. It is difficult to ascertain the amount actually made available for other than defence purposes from the Western nations under the various aid schemes, such as the Colombo Plan, the Colonial Development projects, technical aid through the United Nations, and through various bilateral agreements. Though quite considerable, the amount is almost certainly only a fraction of the above requirement.

Since, of all the former dependent countries, India has the best worked-out plan of social advance and, moreover, is much the largest, it will be convenient to discuss her situation in some detail. Many of the problems facing India are common to other underdeveloped countries, whether in South-East Asia, Africa or South America. The Planning Commission in New Delhi is aided by a powerful statistical and economic group in Calcutta, presided over by a mathematical statistician, Professor Mahalanobis, Fellow of the Royal Society of London. This research group has been assisted by economists from both Western and Soviet countries in the study of the problem of how India can most easily, quickly and painlessly emerge from the nearly static and essentially pre-industrial state in which she found herself on attaining independence in 1947.

During the period of the first Indian five-year plan ending in 1956, the national income rose by 18 per cent. This is an important, even if precarious achievement, in marked contrast to the stagnation of the previous decades. In the second five-year period, 1956–61, a rise of national income of 25 per cent is planned, that is, 5 per cent a year. Whether India can achieve this without additional external financial aid is now doubtful, especially in view of her present large deficiency in her balance of payments. The plan involves not only attaining the high rate of new productive investment of at least 10 per cent of the national income, but also an annual return of 50 per cent in terms of additional output on the capital invested. Such a high net marginal return on capital can only result from very wise planning and efficient execution of the overall pattern of development. Allowing for the population rise of 1·3 per cent a year, the planned rise of income a head is 5 per cent minus 1·3 per cent or 3·7 per cent a year.

Such a rate of rise, *if achieved*, would be nearly twice the average of Western Europe at the present time. So the *relative gap*, that is, the ratio between the living standards of India and of Europe, would slowly begin to close. But how slowly! If these rates continued, nearly half a century would elapse before the living standard of India would climb from one-tenth to one-fifth of that of Europe. Moreover, the *actual* difference between the living standards, that is, the *absolute gap*, would still widen; for instance, if the assumed rates of advance are maintained for the next ten years, the *rise* of annual income of a European will be much more than the *final* income of an Indian.

The limit of India's possible rate of advance is set mainly by the available capital for productive investment, and particularly of foreign exchange to buy production goods from other countries. Failing massive external aid, the advance is limited to what a democratic parliamentary government of a very poor country can raise internally by taxation and loans.

It is clear that the population problem looms especially large to those who are

pessimistic about the possibility of rapidly increasing material wealth, including food. Historically the population of Europe in the nineteenth century rose faster than that of Asia, but no population or food problem arose because wealth, including food, rose much faster. The population of the U.S.A. today is rising faster than that of India. As we have seen, the rise of population in such countries as those of South-East Asia does add, in a defined and calculable way, to the cost and burden of achieving economic progress. However, it is a not uncommon error among Westerners to assume implicitly that no marked economic and agricultural progress in these countries is possible, and so to be led to view them wearing social spectacles through which little can be seen but millions of brown babies.

It is interesting to note that both the governments of India and of China have started to take steps to reduce the population rise. Success in these movements towards reducing the rate of rise of population can only be achieved by education, which itself depends on increasing wealth, and this can only come from industrialisation. Moreover, increasing industrialisation, as part of a general social advance, is the key to increased efficiency of agriculture and so to a greater food supply. Rural education, transport, power supplies, irrigation, agricultural machinery, fertilisers, pesticides and improved seeds are the essential factors.

IV

What is the historic origin of the astonishing differences today between the economic and social conditions of the West and the East? A whole host of hypothetical causes have been advanced: differences in natural ability, climate, health, food, natural resources, religions, social systems or methods of government. To attempt an answer to this problem, it is necessary to look deeply into history.

Archaeologists and historians have now made it possible to grasp the rough outlines of the history of technology. Since this word means 'the science of the industrial arts', and since it is by these arts that the material goods of life are produced, the history of technology can almost be equated with the history of the material aspects of civilisation. With the recent publication of the first two volumes of the *History of Technology*, with Charles Singer as editor-in-chief, the layman can easily survey in some detail, though with many gaps, the history of clothes, textiles, jewellery, cities, houses, temples, arms and armour, vehicles, domestic animals, crops, writing, sculpture and pottery, from the emergence over 5,000 years ago of civilised urban life in the river valleys of the Nile, Euphrates and Indus, up to the present day.

Abundant evidence proves the high level of technological achievement at a very early date. The astonishing building feats of the Egyptians and Mesopotamians are world-famous. Less widely known are the gigantic town-planned cities of Northern India, such as Mohenjo Daro which flourished over 4,000 years ago, with their main drainage systems, vast granaries, and citizen's houses far better than those lived in by most Indians today. Innumerable examples abound in our museums or are illustrated in our histories, demonstrating the astonishing technical triumphs of antiquity. The stern beauty of the 4,000-year-old bronze head of Sargon the Great, or the overpowering grandeur of the 3,000-year-old inner coffin of Tutankhamen, alone suffice to prove that the highest artistry was allied to superb technical skill. The modernity in design of many articles of domestic use in ancient times is most striking. A wooden chair or a manicure set from the Egyptian New Empire of 1000 B.C. would not look out of place in the most up-to-date shop-window today, and might well be better made. A high level of domestic culture was by no means restricted to a few monarchs and officials. The cities of Crete, Babylonia and Egypt around 2000 B.C. must have abounded in comfortable and cultivated people who led very pleasant lives, almost as refined and luxurious as prosperous people enjoy today. In the material arts of life one is constantly impressed by the absence of

many major improvements between the height of the great Middle and Far Easte Empires and the rise of modern technology in Europe about the eighteenth centur A.D. Except for the last 200 years, Rudyard Kipling was not far wrong when he wrote:

> 'We tell these tales, which are strictly true,
> Just by way of convincing you
> How very little, since things was made,
> Anything alters in any one's trade.'

Slow and steady progress, of course, occurred in some fields, retrogression in others. Amongst the most important technological innovations since ancient times were firearms, the magnetic compass, printing with movable type, the horse collar, improvements in wind and water mills, and ocean-going ships. The first three were European medieval developments of much older Chinese discoveries, which had never before been exploited fully. They all predated the scientific revolution which only began around 1600 and thus belonged to the ancient craft tradition.

The contribution of China to the history of technology is only now beginning to be revealed, particularly by Joseph Needham and Wang Ling in *Science and Civilisation in China*. Far Eastern technology appears to have been supreme between 500 B.C. and A.D. 1500. Belief in Europe of the technological superiority of the Far East was indeed widespread from Marco Polo in the thirteenth century to the French missionaries of the eighteenth. The wealth of the Indies was proverbial. Venice—'once did she hold the gorgeous East in fee'—grew rich by trading in textiles, spices, porcelain and jewels.

During the first 200 years of modern science, from 1600 to 1800, science learnt much from technology, but taught it relatively little. The empirical industrial arts were already so highly developed, and indeed had been so for thousands of years, that systematic science had to develop far before it could improve decisively on pre-scientific technology. Even today, the biochemist cannot teach a chef how to cook a better omelette. Despite the interest of the Royal Society in the 'useful arts', it was not until the last quarter of the eighteenth century that the impact of science on technology began to be decisively important.

The vast developments from the seventeenth century to the present day have been almost exclusively a Western achievement. Though the technological foundations on which Europe built arose mainly in the countries of the Near and Far East, these have hardly as yet shared in the later scientific and industrial revolution.

To the question why what happened did happen, I do not think there is an agreed answer. Certainly it had nothing to do with any inherent European superiority—indeed, the history of the previous millennia might well have suggested the opposite. Most probably it was differences in social and economic organisation which were the decisive factors.

Amongst the positive factors was certainly the growth after the eleventh century of the city states of Italy, followed by the free cities of Northern Europe, where merchant entrepreneurs were free from many of the restrictions of feudal society. Not noble but not poor, these men, much like the Ionians of Thales' time 2,000 years earlier, had a pecuniary interest in exploration and in technical innovation. This positive drive towards technological and social change in Europe might not have been sufficient to produce much more than a temporary spurt of pre-scientific technological advance had it not been for the scientific revolution which in the seventeenth and eighteenth centuries grew out of it.

An essential prerequisite for the scientific revolution was the weakening of the opposition of organised religion to the free play of scientific experimentation and speculation. As R. H. Tawney has shown, by the last half of the seventeenth century Europe was weary of a century and a half of religious strife. Religious tolerance first

grew in the merchant cities, where it was found essential for trade. Intellectual tolerance followed in its wake. When the Civil War in Britain ended with the victory of the technologically progressive cities and merchant classes over the still feudally inclined monarchy and aristocracy, Britain was free from restrictions both on scientific thought and on the use of money for profitable and speculative investment. The way was clear for Britain to become the first industrial nation and to usher in our present scientific-technological age. However, even in these favourable circumstances the necessary conditions for the take-off did not exist till the end of the eighteenth century. The parliamentary system of government, which is Britain's proudest political achievement and still a major export, arose in mature form out of this long struggle against privilege and restriction.

One notices that the conquest of Asia and America by Europe was largely completed before the scientific revolution. It was Western superiority in the pre-scientific technology of firearms and ocean-going ships which formed the material basis of the rise of the European empires in Asia and America. Europe was then passing through one of those periods of intense and ruthless energy, both mental and physical, which occur, for complex reasons, from time to time in the history of mankind.

Just when Europe was in a crusading, expansive and aggressive mood, the once great Indian Mogul Empire began to disintegrate, as so many empires have done in the past. Portugal, Holland, France and Britain fought long and bitterly for the spoils. Finally India, Burma and Ceylon fell to Britain, Indonesia to Holland, and Indo-China to France, to be governed from Europe till they acquired their independence after the Second World War. China, though humiliated and partially occupied, never came completely under Western dominance. The vast Chinese Empire seems to have been too rigid to change of its own accord, but survived mainly because of the bitter rivalries between the Western Powers. Japan alone of Asian nations both accepted Western technology and avoided Western dominance. In fact, for a time she beat the West at her own game by achieving a more rapid rise in income than any West European country before or since, and reached a wealth a head at least as high as that of the poorer countries of Europe. This success had a profound effect in those Asian countries which were struggling at that time to be free from Western dominance.

The impact of the West on some of the static or declining pre-industrial civilisations of Asia brought about certain fundamental changes essential to social advancement, including the weakening of the power of the old feudal aristocracies, the growth of a merchant class, the introduction of Western education, science and medicine, and in some cases the beginnings of a modern industrial and transport system. However, in no Asian country except Japan was the take-off achieved.

It is notoriously hard for Westerners to regard with detachment the long period of European ascendancy in the East. With the fading of the ascendancy, objectivity is becoming easier, and the West has no longer any excuse for not appreciating the Asian viewpoint. The Indian scholar-diplomat K. M. Pannikar in his recent book *Asia and Western Dominance* tells the story of the relations between East and West from Vasco da Gama's landing at Calicut in 1498 to the present day. The reader who meditates on some aspects of the story will be inclined to adapt an old aphorism originally applied nearer home and say 'History is something that Europeans should remember and Asians should forget.' In truth most Asians have, or politely pretend they have, and this is in no small part due to Britain's freeing of India, Pakistan, Ceylon and Burma after the War: in my view, one of the greatest single acts of British statesmanship of this century. It is, I think, a tribute to both the Indian and the British peoples that after decades of bitter political struggle, personal relationships between Indian and British individuals are closer than ever before. It is important, however, to remember that most South and South-East Asian countries

attribute much of their present backwardness to colonial rule. This was a constant theme at the recent conference at Bandoeng of the representatives of 1,500 million coloured people.

V

After 2,000 years of relative stagnation of world technology, Europeans staged a scientific, technological and industrial revolution, which has transformed within three centuries, and is still transforming, both the material way of life and the mental outlook of mankind. This astonishing achievement of the peoples of Europe must rank with the greatest events of world history. During the centuries when European scientists, technologists and craftsmen were conquering nature, European fighting men, missionaries, traders and administrators were conquering the world. By the close of the nineteenth century a hundred million Europeans were ruling some 700 million people in Asia, Africa and America.

Understandably, this double and interrelated triumph of the West over both nature and the world went to its head. By the last half of the nineteenth century the doctrine of the innate superiority of the white peoples, with its corollary of the inferiority and incapability of coloured people, had become widely accepted in the West. To a great extent, white racialism is economic and administrative in origin. Men tend to feel superior to those whom they rule and to despise those whom they exploit.

What has science to say of the innate mental differences between the different races of mankind? Rather little, it seems—at any rate—so far. The physical differences of colour, features and stature are obvious without the aid of scientific study. It may well be that innate differences of mental make-up do exist: but scientific study has failed as yet to establish their existence or nature. Moreover, even if mental and temperamental differences could be demonstrated as innate in such qualities as courage and determination of purpose, physical strength and skill, or in musical, literary, artistic, mathematical and analytical power, between the individuals of different races, the attempt to rate different races in an acceptable general order of merit is foredoomed to failure. For no agreement would be obtainable on the relative weight to be attached to the various abilities: indeed, the various human qualities have different values in different environments.

Even if further studies should show marked differences now between different races, it would be as dangerous to extrapolate too far forward into the future just as the history of technology has shown it to be dangerous to extrapolate back into even the relatively recent past. The artistic and technical triumphs during the Middle Ages of Peru, Mexico, India, Indo-China and China are a perpetual reminder that great creative ability and immense energy have appeared quite recently among many different peoples, with diverse social systems and religions, and vastly different climates. The grandeur of Cusco, Chichen-Itza, Fatehpur-sikri, Angkor Vat and the Forbidden City in Pekin are permanent memorials of not very distant achievements of the coloured peoples of the world.

I am convinced that Western pessimism about the possibility of social advance by ex-colonial Asian countries is both unjustified in fact and a serious cause of practical error. It has led the West to overestimate the need of the have-not countries for technical advice from Western experts and to underestimate the need for simple financial help. Admirable as many of the schemes of technical aid have been, particularly those in education, medicine and agriculture, the sending of experts to poor countries without the capital to carry out their plans, could be as irritating as to send a trained cook to a family unable to pay the baker. In so far as expert aid by Western technical personnel is needed by an underdeveloped country, it is usually both more efficient and cheaper to provide money for the country itself to hire those whom it needs. Unless followed up by massive financial help, some of the West's

present aid programme may yet merit the war-time wisecrack of 'offering all aid short of help'.

What would constitute real help? The figures already given suggest that in addition to likely commercial and government short-term lending an additional £1,000 million a year is needed as a free gift or as long-term loans from the 400 million rich Westerners to the 1,000 million Asians, Africans and South Americans in the underdeveloped countries outside the Soviet orbit. To the Western donors, this would amount to a levy of less than 1 per cent of their income; Britain would pay £150 million a year, and this would only postpone by less than a year the expected rise of 50 per cent in British living standards over the next quarter of a century.

To the recipient country, it would mean £1 a year a head in foreign exchange, that is, a 5 per cent addition to their income of £20 a year. If wisely invested, this should allow the standard of life to rise at rather more than an extra 2 per cent a year. It would therefore make an appreciable contribution to the outstanding problem of the now widening gap.

Such aid to a nation with a well-developed national plan would probably not be needed for more than a limited period, perhaps one or two decades. By then she should have achieved the take-off and be in a state of sustained economic growth, able to dispense with outside aid. Extending the aeronautical metaphor, the suggested Western action would amount to using its great wealth and material power to give the have-not countries an *assisted take-off*. It must always be remembered that without a strong and intelligently directed internal drive for social betterment, no country can achieve much. Outside aid can only help fully those who are already prepared to help themselves to the limit of their capacity.

Some may think I have taken a too severely economic attitude to the problem of the have-not countries and have neglected the more spiritual factors. Undoubtedly the citizens of a static pre-industrial country may possess many qualities such as contentment, humility, kindness and gaiety in greater measure than those of a rich and progressive country. Though this is true, it is a doctrine which the restless and prodigal West might well preach to itself rather than to others. With nations, as with individuals, the ultimate hypocrisy of the rich is to preach the virtue of poverty to the poor.

VI

The essential fact of the present-day non-Soviet world is that the huge gap of material living standards between the industrial and the pre-industrial countries is steadily widening. Moreover, most new technological advances tend to widen it still further. If present tendencies were to continue for many decades, then one would find many of the ex-colonial and similar countries still largely pre-industrial and so very poor. On the other hand, the vast acceleration of technological progress in the West will be leading the most advanced of the industrial countries to pass progressively, if they so wish, through the phase of the four-day week, on the road perhaps to the final bliss, or ultimate boredom—according to temperament—of the five-day weekend. The difference today in material wealth cannot be primarily attributed to differences of national or racial aptitudes, but has complex historical origins. Moreover, the gap in wealth is now so great that an important contribution to narrowing it could be achieved at a relatively small cost to the rich industrial countries.

In turning to consider whether the Western world should make this sacrifice, we leave the field of social analysis and enter that of political and moral controversy. Some people will stress the long-range political dangers of the widening gap and that the survival of parliamentary systems of government in many of the underdeveloped countries may be dependent on the achievement of rapid social progress. Others will lay most weight on military considerations, arising out of the polarisation

of so much of the world into Soviet and anti-Soviet blocs. If the West does not help, perhaps the Soviet bloc will. This has hitherto been by far the strongest stimulus to what Western aid has been forthcoming. Strong economic arguments can be produced: poor countries are also poor markets for manufactured goods, and may be unreliable suppliers of essential raw materials: such 'enlightened self-interest' is particularly relevant for Britain with her essential dependence on overseas trade. Some of the strongest motives for a substantial aid programme arise from moral and religious values. Many feel that such large differences of wealth, health and opportunity are wrong, and that to work to reduce them would give a new sense of moral purpose to the West. Recently Ritchie Calder has aptly suggested that massive Western aid to Asia might be considered as reverse lend-lease for the incalculable debt which the West owes to the East for the age-old gift of the great tradition of empirical technology which was the essential first basis of Western triumphs. One might add—for it is often forgotten—that all the great religions of the world are of Asian origin.

Here are motives enough for each of us to weigh and make his choice. My own view is clear. I think the West should make the great experiment of sacrificing some of its immediate prosperity to give massive aid to the have-not countries. To give this aid would not, I believe, prove very painful and it may do us a power of immediate moral good and, ultimately, of material good as well. If the major nations of the West cannot agree quickly to provide the required £1,000 million, then I would hope that Britain would 'go it alone' and make her contribution of £150 million a year available to her former colonies, in addition to what she is already doing. In holding this view, I do not forget the many economic and social problems nearer home which need urgent attention.

Scientists and technologists have a special responsibility in this matter, since it is their genius and their skill which alone can bring the material basis of happiness within the reach of all. The progress of the natural sciences, the West's greatest achievement, has been based on experiment. Let us make now this great social experiment to spread the benefits of our labours. At present they reach only a few. A have-not country, bound like a modern Tantalus by the chains of its lack of capital, gazes with unquenchable thirst on the growing riches of modern technology which it cannot enjoy.

Times and problems have changed since Humphry Davy commented so shrewdly on the social state of Britain at the very start of the industrial revolution. I will end this lay sermon—for this is what this address has become—by attempting to transmute his words to suit the contemporary scene:

The uneven division of power and wealth, the wide differences of health and comfort among the nations of mankind, are the sources of discord in the modern world, its major challenge and, unrelieved, its moral doom.

STELLAR ENERGY AND STELLAR EVOLUTION[1]

ADDRESS BY PROFESSOR T. G. COWLING, F.R.S.

PRESIDENT OF SECTION A

IDEAS of stellar evolution have exercised the minds of natural philosophers ever since the Aristotelian picture of unchanging heavens was abandoned in the seventeenth century. In the eighteenth century, Kant and Laplace advanced the hypothesis that a star was formed by the contraction of a primeval nebula, planets being formed from parts of the nebula left behind during the shrinkage. As an alternative hypothesis, Buffon proposed that planets might be formed from lumps torn from the parent star by some catastrophic process; numerous variants on this hypothesis have been considered by later authors. The equilibrium of rotating fluid masses was also first studied in the eighteenth century by Maclaurin, though it was not until considerably later that the origin of double stars was being explained by the fission of a parent star, due to its excessive rotation. Up to the later years of the nineteenth century, cosmogonic theories in the main relied on purely dynamical processes, and the origin of the solar system was the problem nearly always studied.

The situation took a new turn with the increasing application of photography and spectroscopy in astronomy at the close of the last century. In the first years of this century, a decisive step was taken when Hertzsprung and Russell first constructed a diagram (the Hertzsprung–Russell, or H.R. diagram) on which were plotted the absolute brightnesses of the stars against their colours or, what is roughly equivalent, their spectral types. The type of diagram which they found is illustrated in Fig. 1, in which each point represents an individual star. On this diagram the stars fall into two sequences. The main sequence, containing most of the stars, begins with very bright blue stars and passes through stars of steadily decreasing temperature and luminosity to faint red stars. The second or giant sequence consists wholly of bright stars; the luminosity does not vary greatly along this sequence as one passes from white to red stars, the decrease in temperature being compensated by an enormous increase in the radius.

Later work showed that main-sequence stars, especially the fainter ones, are very closely grouped round a single track in the H.R. diagram, and that the mass and radius decrease with the brightness down the sequence. The giant sequence was found actually to consist of very few stars, though because of their brightness greater importance was at first attached to them than their number really warranted. So far as actual numbers are concerned, the class next in importance to the main-sequence stars was found not to be the giants but the white dwarfs. This is a class of stars of fairly high surface temperatures, but giving very little light because their densities are enormously high and their radii correspondingly tiny.

The existence of clearly defined sequences invited an interpretation in evolutionary terms. Hertzsprung and Russell suggested that newly-born stars first become visible as vastly distended red giants; they evolve along the giant sequence, and on to the main sequence, by contracting and increasing their surface temperatures. At first they consist of perfect gas throughout, but ultimately the density becomes so high that the perfect-gas laws no longer apply, and the rise in temperature is checked. After this the star cools down more or less like a solid or liquid mass, and

[1] Address delivered at the Dublin Meeting of the British Association on Friday, September 6, 1957.

passes down the main sequence, glowing steadily less brightly until at last it is extinguished.

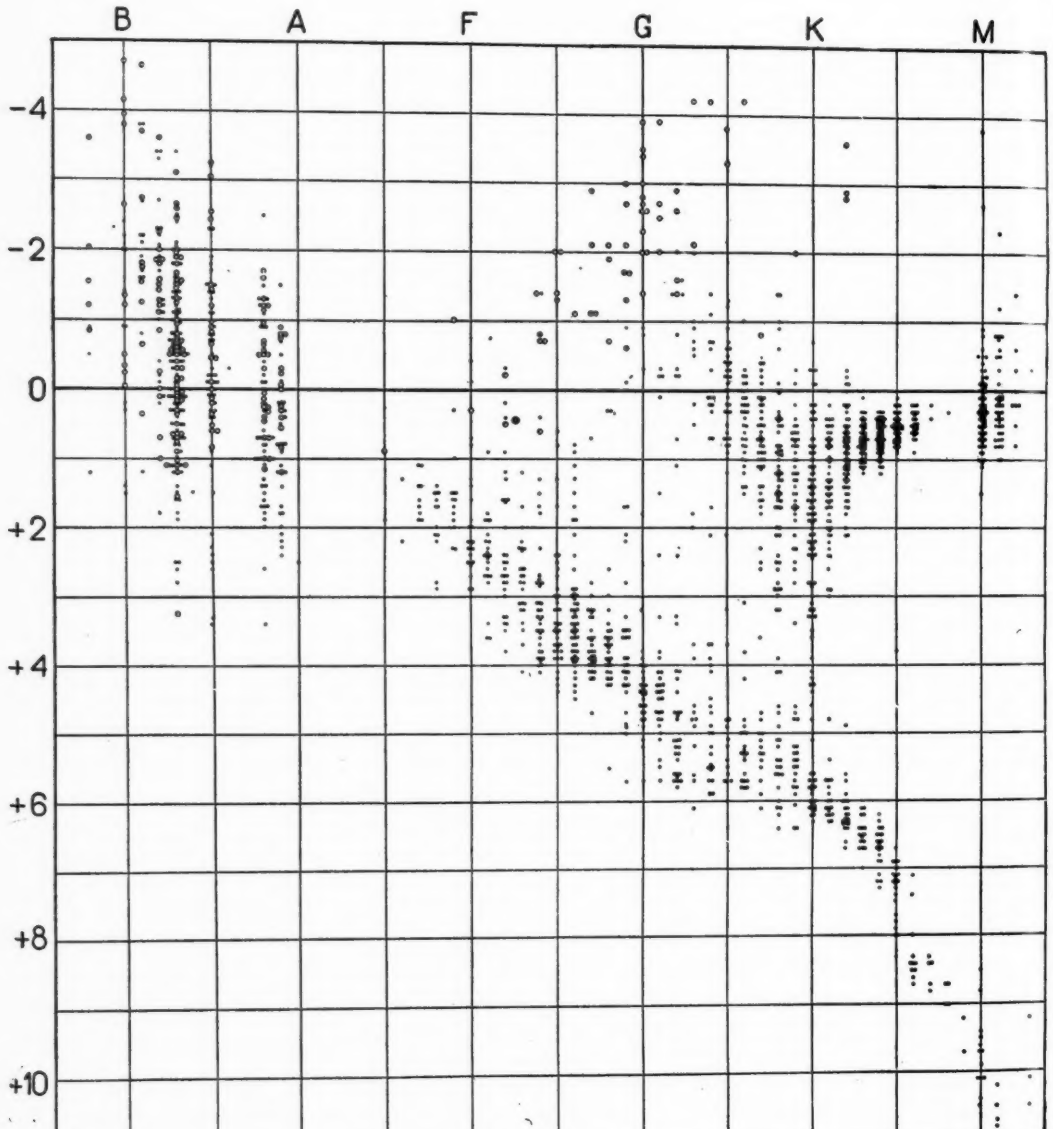


Fig. 1.—An early Hertzsprung-Russell diagram. (Reproduced from Eddington's *Internal Constitution of the Stars* (1926), p. 175, by permission of the Syndics of the Cambridge University Press.)

The ordinate is absolute magnitude (decreasing magnitude means increasing luminosity), the abscissa is spectral type. The colours corresponding to the different spectral types are shown below the diagram.

This giant and dwarf theory was abandoned about 1925. Eddington's theoretical work showed that in spite of their high densities most main-sequence stars could be regarded as composed of nearly perfect gas. Thus the difference between giants and main-sequence stars was not to be explained in terms of the difference between

a perfect gas and material much more like a liquid. However, Eddington was still prepared to accept the possibility that giants are stars evolving on to the main sequence, though he left open the question whether there is evolution down the sequence due to loss of mass.

Since that date, nearly every possible combination of directions of evolution along the two sequences has had its supporters. Evolution of stars down the main sequence, being from greater masses to less, must presumably be accompanied by the progressive ejection of material from the surface; those who believe such ejection to be important have included Kuiper and Struve in America, and a number of Russian astronomers. On the other hand, Hoyle and Lyttleton have suggested that the very bright stars at the top of the main sequence are formed from much less luminous main-sequence stars which have wandered into dense clouds of interstellar material and have grown great by the gravitational capture of this material. Again, giants were originally supposed to be newly-born contracting stars, and indeed newly-born stars must contract for a while; but in these days giants are supposed to grow bigger as they evolve, not to shrink.

The different points of view which have been advanced have all to a greater or less extent been based on observation, and the observations alone are not enough decisively to distinguish between them. Thus theories of stellar evolution were, until a few years ago, regarded as mainly speculative. Soundness in astronomy was thought to consist in ignoring such speculations and simply accepting the stars as they are. Only when, twenty years ago, the sources of stellar energy began to be understood, was the clue found by which the evolutionary problem could be solved.

The problem of stellar energy, like that of stellar evolution, is one that has been studied for many years. In the eighteenth century, when heat was imperfectly understood, there was no problem; the authors of science fiction wrote of the sun as a superplanet surrounded by glowing clouds under which superbeings carried on an existence not unlike ours. In the nineteenth century, after conservation of energy had been recognized as a law of nature, a source had to be sought for the copious heat which the sun is continuously pouring forth. Lord Kelvin, after showing that energy due to meteoric impacts could heat the sun sufficiently only if mass were being captured at an impossibly high rate, advanced with Helmholtz the theory that the energy is gravitational in origin, being provided by the contraction of the sun as it cools. This provided energy enough to keep the sun going for only a few million years, far less than the times demanded by geologists and biologists for evolution on the earth; but it was the most that nineteenth-century physics could provide.

The first suggestion that stellar energy could be generated by subatomic processes was, I believe made by Jeans about fifty years ago. Ordinary radioactive materials, in spite of their intense activity in an atomic bomb, cannot give anything like enough energy to keep a star radiating. Jeans therefore suggested that fairly stable transuranic elements might exist in the stars, and that their decay might liberate far more energy than radioactive materials on earth. In his *Internal Constitution of the Stars*, published in 1926, Eddington speculated, in the light of the astronomical evidence, on the different energy-generating processes which seemed possible. He was misled to some extent by a rough calculation which indicated that a star must be unstable if the rate of generation of energy increases too rapidly with the temperature. The suggestion was not that in this case the star is in danger of blowing itself up like an atomic bomb, but that it would be set oscillating with steadily increasing violence. I was able to show, about ten years later, that dissipative effects in the outer layers of a star are enough to remove the danger of this kind of instability. Nuclear generation of energy does not, in fact, endanger the continued existence of normal stars.

Eddington considered two possible modes of nuclear generation of energy, one being by a process of mutual annihilation of protons and electrons, the other by a process in which protons and electrons combined to form helium nuclei. Each process converted mass into radiation energy, in accordance with Einstein's well-known equation $E = mc^2$. The annihilation process produced the most energy; it made use of all the mass, whereas in the helium-building process the mass converted into energy is only the small excess of the mass of four protons ($4 \times 1{\cdot}0075$ units of atomic weight) over that of a helium nucleus ($4{\cdot}002$ units).

Eddington expressed a cautious preference for the annihilation process. It implied the complete conversion of matter into energy, which permitted evolution down the main sequence. Also the less generous supply of energy from helium-building implied a rather skimpy time-scale unless nearly all of a star's mass were initially hydrogen, which did not at the time seem reasonable. But the strongest argument against the helium-building process was that the only mode of building a helium nucleus seemed at that time to be a multiple collision of six particles, four protons and two electrons. Such collisions seemed almost indefinitely improbable; however, Eddington was careful to point out that the simultaneous existence of hydrogen and helium in the universe appeared to imply that some means existed for the transmutation of the one into the other, so that too much weight could not be attached to the apparent improbability. Eddington's final comment deserves a special mention, in view of developments to be described later; it was that the helium-building process would lead to no interesting astronomical consequences.

For it is the helium-building theory that now holds the field. No mechanism for the annihilation of matter has yet been found; but the discovery of the positive electron and of the isotopes of hydrogen and helium has removed the difficulties about helium-building. In 1938 and 1939, Bethe and von Weizsäcker independently suggested two helium-building processes which can function at stellar temperatures. The first was the so-called carbon–nitrogen cycle, in which a carbon nucleus is penetrated by successive protons, emitting positive electrons when necessary, and is transformed in turn into the carbon isotope of atomic weight 13, and the nitrogen isotopes of atomic weight 14 and 15. The cycle ends when a further proton enters the last nitrogen isotope, since fission then occurs, giving back the original carbon nucleus and also providing a helium nucleus. The net result of the cycle is the conversion of four protons into a helium nucleus, with corresponding generation of energy; the carbon nucleus acts as a sort of catalyst.

The second process considered was the direct building of helium by the combination of protons without any catalyst. The first step is the building of a deuteron (heavy hydrogen nucleus) by the collision of two protons, with the emission of a positive electron. Then follows very quickly the capture of a further proton, resulting in a helium nucleus of atomic weight 3. Nuclear science could not decide in 1939 what was likely to be the final reaction leading to the production of a normal helium nucleus. This was not regarded as important; whatever the reaction might be, it was almost certain to be far faster than the original deuteron-forming reaction, whose rate must accordingly be the main factor determining the rate of helium-building. It was more important that, whereas all the other reactions considered could be reproduced in the laboratory, no experimental check on the theoretical speed of deuteron formation was possible. Indeed, some doubt existed whether deuteron formation was not forbidden by the selection rules of nuclear theory. Only after about ten years was this doubt finally removed. The final reaction in the helium-building was not identified until 1951; it is a very fast reaction in which two helium nuclei of atomic weight 3 react to give a normal helium nucleus and two protons.

All the reactions, of both the proton–proton and carbon–nitrogen cycles, are in a certain sense forbidden; a proton has to penetrate a nucleus in spite of having

insufficient energy to get through its electrostatic repulsion. Quantum theory does permit such penetration, but with a small probability; the probability drops off with ever-increasing rapidity as the proton energy falls to values small compared with that of the electrostatic field. Thus, for example, at temperatures like those met at the centres of stars the rate of generation of energy by the carbon–nitrogen cycle varies about as fast as the fifteenth power of the temperature. The repulsion between particles is less important in the proton–proton process; this would be much faster but for the fact that deuteron-building demands not only that two protons are brought close together, but that a positive electron is emitted while they are close together. The reduced importance of electrostatic repulsions means that the generation of energy by the proton–proton process varies more slowly with the temperature—roughly as the fourth or fifth power. Reactions involving nuclei heavier than oxygen are not important in normal stars, because of the same energy-effect.

Bethe assumed a nitrogen abundance of 10 per cent by mass, consistent with the current estimates of stellar composition. He found that the carbon–nitrogen cycle then gave just about enough energy to explain the emission of main-sequence stars down to about one-tenth of the sun's brightness. Still fainter stars were sufficiently cool throughout for the carbon–nitrogen cycle, with its strong temperature-dependence, to yield place to the proton–proton process.

More recent work has modified this picture. It showed that Bethe underestimated the rate of generation by a factor of 7·5 for the proton–proton process, and an even larger factor for the carbon–nitrogen cycle. Thus the nuclear processes are able to provide sufficient energy at a lower temperature, at which the carbon–nitrogen cycle, with its strong dependence on the temperature, is relatively less important. It was also found that Bethe's estimated nitrogen-abundance was far too high; most of the material of normal stars is hydrogen, and not more than one or two per cent of the mass, all told, consists of atoms heavier than helium. For these reasons, the carbon–nitrogen cycle is less important than was originally supposed. It probably is the main energy source for stars appreciably more massive than the sun; but the sun itself, and all smaller stars, generate most of their energy by the proton–proton process. The temperature at the centre of the sun is, according to recent estimates, some 15 million degrees K; that in very bright stars is a few million degrees more. The very bright stars, because of the strong temperature-dependence of the carbon cycle, generate nearly all their energy in a central core, in which the heating leads to vigorous convection currents; the less bright stars have no such 'boiling' core.

The facts about stellar energy clear our ideas on stellar evolution, first of all by giving us a time-scale. The total energy available from helium-building can be calculated from the mass lost in the process. For example, it can be estimated that, if the sun initially was composed almost entirely of hydrogen, helium-building could keep it radiating at its present rate for a little over 10^{11} years before all its hydrogen would be used up. This is amply bigger than the estimated age of the solar system (4 or 5 $\times$ 10^9 years). On the other hand, a star ten times as massive as the sun is more than 2000 times as bright, so that its rate of generation of energy per unit mass is more than 200 times the sun's. Thus it would use up its hydrogen in not much more than 5 $\times$ 10^8 years, considerably less than the age of the solar system. The evolutionary lifetime may be even smaller than this. The hydrogen is burnt into helium near the star's centre. The helium—the 'ash' of the process—is heavier than the hydrogen, and so is unable to rise out of the core. Thus no fresh material can descend to replenish the star's nuclear furnace, and the star's properties may be seriously modified long before its hydrogen is all used up—for a star ten times as massive as the sun, perhaps in less than 10^8 years.

A very bright star must, then, be far younger than the sun for its energy-sources

not to be exhausted, and younger still if it is not to show clear signs of evolutionary changes. That is, very bright stars are those among which one would expect to find most evidence of evolution. Unfortunately, one's search for such evidence is complicated by the possible presence, in any part of the heavens, of stars of different ages, some of which show signs of evolution and some not. It is desirable to be able to compare the properties of bright and faint stars within a group of stars all more or less of the same age. Such a group is provided by one of the great star clusters of the Galaxy.

Astronomers in these days agree that the Galaxy, or Milky Way, is a spiral nebula like the great Andromeda nebula. It has a central nucleus, a mass of star clouds which can be seen in the direction of the constellation Sagittarius; also it has been possible to identify parts of two or three of its spiral arms, which are picked out by very bright stars and clouds of interstellar material. The sun is on the edge of one of the spiral arms. We see the Galaxy as a belt in the sky because the stars which compose it are largely concentrated close to a plane, and we are looking at them from a point not far from that plane. Numbers of clusters are associated with the Galaxy. They are divided into two groups, the galactic clusters, which are concentrated close to the galactic plane, and the globular clusters, which indeed are grouped round the nucleus of the Galaxy, but exhibit little concentration towards the galactic plane.

Globular clusters are held tightly together by mutual gravitational fields; a mathematical discussion readily shows that the loss of a star from a cluster, or the capture of another by it, is a rare occurrence. Galactic clusters are similarly held together, though less tightly, because they in general contain fewer stars. Thus a cluster consists of a group of stars which have been together all their lives. It is natural to believe that they were born nearly at the same time from condensations in a giant cloud of gas and dust. Direct evidence of the possibility of such multiple births is not lacking. For example, very bright blue stars, which must have been born recently, have been shown by Ambartsumian frequently to occur in local groups or associations. Sometimes these associations are expanding; by tracing back the motions of their individual stars one can show that at a certain time, only a few million years ago, they were all very close together, so that their common origin seems plausible. Star clusters similarly can be presumed to have had a common birth, but the stars composing these are so numerous that their mutual gravitational fields have held them together.

The first H.R. diagrams for globular clusters were obtained only a few years ago. In them (cf. Fig. 2), the top of the main sequence, corresponding to the bright blue stars, is found to be entirely absent. Any bright blue stars that were originally present have burnt up so much of their hydrogen that they have evolved into new forms. The points of the H.R. diagram representing the individual stars are fairly closely grouped round a single curve. This curve travels along a main sequence in its lower parts, though perhaps it is a little below the main sequence of Fig. 1. However, when it reaches stars only a little more massive than the sun, the curve turns sharply to the right, and diverges from the main sequence. It then climbs up into the region corresponding to the giant stars. Finally it appears to return on its tracks and follow a more horizontal course across the region that would be occupied by the higher part of the main sequence.

The H.R. diagram for a galactic cluster similarly approximates to a single curve. This usually, but not invariably, follows the main sequence up to a point well above the point where the curve for globular clusters swings to the right; for some galactic clusters, indeed, the curve follows the main sequence nearly to its highest point. But occasionally, like the curve for globular clusters, it branches off the main sequence; it turns sharply right and then climbs into the giant region, though less high than the curve for globular clusters. There is sometimes a faint indication of

grouping of points round a nearly horizontal return branch, well above the main curve, as for globular clusters; but for galactic clusters the curve tends to be less clearly defined in the upper regions than below.

The theory of stellar structure (as developed in the past few years by Schwarzschild, Hoyle, and their co-workers) has been able to explain these properties in terms of helium-building. After an initial period of contraction, a star settles down at a point of the main sequence. It first burns up all the hydrogen near the centre, so producing an exhausted core. Later it generates energy in a shell immediately surrounding this core; the core steadily grows as more and more material is

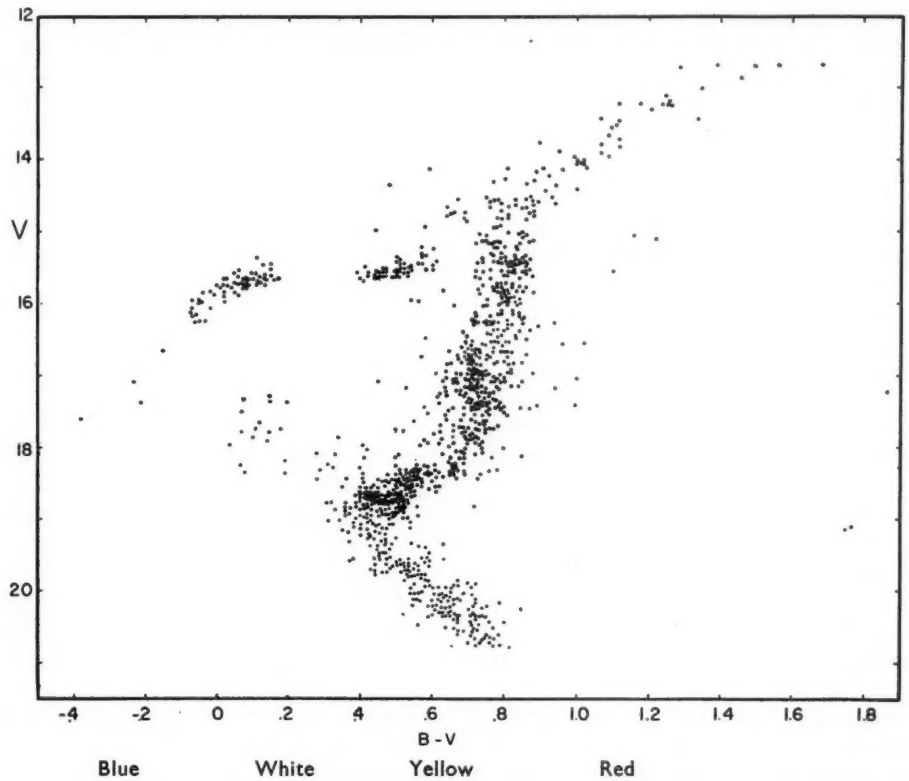


Fig. 2.—The H.R. diagram for the globular cluster M3. (Reproduced by permission from *Monthly Notices of the Royal Astronomical Society* (1956), vol. 116, p. 528.) The ordinate is the visual magnitude (differing from the absolute magnitude by a constant); the abscissa is a colour-index, positive for yellow and red stars, negative for blue stars.

exhausted. During the first stage of the burning the properties of the star do not alter greatly; the radius and brightness both slowly increase, but the effect is only that the star climbs a very little way up the main sequence. An exhausted core begins to form when the star has burnt about ten per cent of its hydrogen. The appearance of the core makes the star's radius increase far more rapidly; its brightness is at first less affected, so that the star reddens and moves to the right of the main sequence. By the time that twenty per cent of the hydrogen is exhausted, the radius has increased to three or four times its starting value. Here the increase in radius is checked somewhat because the outer layers begin to be cooled by convection and so expand less; however, the central temperature steadily increases, and with it the star's energy-production. Thus the star moves rapidly upward in the H.R. diagram, and becomes a giant.

In a cluster, the brightest stars first exhaust ten per cent of their hydrogen and begin to swell into giants; as time passes, stars of steadily smaller masses move off the main sequence and go the same way. This explains why the top of the main sequence is missing in the H.R. diagram for globular clusters and for some galactic clusters. The point at which the H.R. diagram for a cluster diverges from the main sequence indicates where stars have exhausted about ten per cent of their hydrogen since their birth. Thus the age of a cluster can be calculated from the mass and brightness of stars at this point. The age of globular clusters turns out to be of the order of 6×10^9 years, similar to that of the solar system but a little larger. Galactic clusters have not all the same age; the oldest are about as old as the globular clusters, the youngest are only two or three million years old. Galactic and globular clusters appear to belong to two different stellar populations.

In fact, Baade in 1944 inferred the existence of two stellar populations in the Galaxy by analogies with the stars of extragalactic nebulae. Type I population includes the galactic clusters and most of the stars near the sun, save for fast-moving stars which are clearly not natives of the locality; it can be expected to be characteristic of the spiral arms of the Galaxy. Type II population includes the globular clusters and the stars of the galactic nucleus. Certain Russian astronomers have urged that the division of the stars into two populations is an oversimplification, and that intermediate types exist; but the concept of two populations is at any rate very useful. Populations I and II differ not only in their distribution but in their composition; whereas type I stars have about 2 per cent by mass of elements heavier than helium, type II stars may have only 0·25 per cent.

The first H.R. diagrams, illustrated in Fig. 1, referred only to stars near the sun, i.e. type I stars. Type II stars are on the whole older than type I, and may be taken to represent the primeval population of the Galaxy. Type I stars, on the other hand, are a mixture of stars of different ages, some dating back nearly to the formation of the Galaxy, and some of quite recent birth. There can be no doubt that type I stars are at this moment being born in the great clouds of interstellar matter which characterize the spiral arms of the Galaxy. We do not see them being born, because they do not heat up sufficiently to be seen until they have already existed for a fair time. But the H.R. diagrams for some very young clusters do show a few rather distended faint stars, which may be stars which have not yet had time to contract on to the main sequence.

According to the ideas just explained, the main sequence is no longer to be regarded as an evolutionary track. It is rather to be regarded as the starting line along which stars line up at the beginning of their evolutionary race. They stay near this line for a considerable time; the sun, and fainter stars of similar ages, have not had long enough to have been able to get far from the line. We have traced the development of massive stars up to the giant stage. But this does not end the story. Massive stars do not escape from rapidly exhausting their sources of energy simply by becoming giants; the converse is true, for a star as a giant may be much more than ten times as bright as initially. Thus the star must evolve quickly as a giant; it could keep going for a considerable time as a giant only if it could call on some new and far more potent source of energy. No such source is known to terrestrial experience, and in any case it is dubious whether one needs to think of ways of keeping a giant going. Giants are relatively rare, and their rarity suggests that no star can exist as a giant for more than a short time. One may ask what is the ultimate fate of a star after its moment of brief splendour as a giant. No completely definite answer can be given to this question.

For the stars of population II the suggestion is often made that ex-giants move to the left along the horizontal branch of the H.R. diagram for globular clusters (Fig. 2); when they reach the left-hand end of this branch they rapidly contract to become white dwarfs. The giants of population I may evolve similarly, but for

population I there is less evidence of a horizontal branch on the H.R.diagram. One difficulty of the suggestion is that the blue stars at the left-hand end of the horizontal branch are only moderately massive; white dwarfs are definitely under-massive. Thus a very massive star can become a white dwarf only if it loses a large part of its mass on the way. Three ways of losing mass can be suggested.

First, when a massive star first reaches the main sequence its surface temperature is high—say 20,000° or 30,000°K. Thus radiation pressure at the surface may be strong enough to drive out spurts of matter with a velocity sufficient for it to escape from the star's gravitational field. The ejection is assisted by the fact that such stars often have large rotational velocities, of order 100 km./sec. or more. Ejection of mass has, in fact, been inferred by some observers from the spectra of certain very bright stars.

Secondly, when a star becomes a giant its great distension means that it has less gravitational control on its outer layers than it initially had. If it is one component of a double star, it may lose mass to its companion; if it belongs to a tight cluster, neighbours may be able in rare cases to capture mass from it. Alternatively it may be able to shed matter unaided, by a sort of evaporation from hot spots of its extreme outer layers. Such evaporation from hot spots is known to occur from the sun, and gives rise to the streams of particles that produce magnetic storms and the auroral displays. It can reasonably be expected to be more important in giants.

Thirdly, the star may emit matter more or less explosively, as a consequence of a nuclear explosion. When a star swells into a giant, it is only the outer layers that expand; the density near the centre may increase 100 or 1000-fold, and the central temperature also rises. If the central temperature reaches 120 million degrees K, energy will be liberated in the core by a new process, in which three helium nuclei combine to form a carbon nucleus. The energy generated by this process per unit mass is only one-tenth that of helium-building, but if it is supplied sufficiently fast there may be something like an explosion. The condition for an explosion is that the central density should be so great that the material is strongly 'degenerate'; this means that if heated it cannot cool itself by expanding adiabatically. Thus, when helium-burning begins, the core heats up and the energy-generation increases enormously (roughly like the thirtieth power of the temperature); conditions resemble those in a nuclear bomb. There may be other, less familiar, nuclear processes which could produce a similar explosion.

Stellar explosions are known to occur in ordinary novae and supernovae. In supernovae the whole star explodes; nuclear reactions of many diverse types may take place in the extreme heat at the centre of the star. Supernovae have evolutionary interest not only as the brilliant ending of one star, but also because they scatter abroad heavy elements which have been built up in the star's central nuclear furnace, to mix into the material available for building new stars. In ordinary novae the star blows off a shell of matter, sometimes several shells. Nova outbursts may be the last step before a star becomes a white dwarf. However, the mechanism suggested above is not the only one capable of producing stellar explosions, and indeed it is extremely unlikely that one single mechanism can be capable of explaining both novae and supernovae.

To sum up, a complete picture of the course of stellar evolution cannot at present be given. The account, given above, of the way in which stars swell up to become giants is fairly secure; in picturing their subsequent development one has to rely to an increasing extent on the crystal ball. However, thanks to the hints given by the theory of nuclear energy-generation, it is now possible to give a fairly reliable picture of parts of stellar evolution, and the known area is rapidly growing. We are in the exciting stage of new discovery, and I hope that I have been successful in conveying to you some of the excitement. This excitement gives one sympathy with an earlier cosmologist (the author of the book of Job), who said that when the foundations of

the earth were laid 'the morning stars sang together and all the sons of God shouted for joy'.

The question may perhaps be asked, what is the bearing of these evolutionary ideas on a religious faith? Wright, of Durham, who anticipated Kant in a nebular hypothesis of the origin of stars, showed in his diagrams the eye of Providence as present everywhere in his nebula. On the other hand the Russians have taken up the study of cosmogony with great energy, presumably as providing an explanation of the origin of the universe not involving the hypothesis of God. I personally prefer Wright's viewpoint; but the fact that such divergent attitudes are possible suggests that the facts on which they are based are not wholly those of cosmogony. Further than this it would be improper for me to go at this juncture.

CHEMICAL APPROACHES TO THE INVESTIGATION OF LUNG CANCER[1]

ADDRESS BY DR. J. W. COOK, F.R.S.

PRESIDENT OF SECTION B

Introduction

I FIND from the records of the British Association that the first meeting of this Section, under its present designation of Section B, took place in Dublin in 1835. So the Section will have some sentimental attachment to Dublin, the city of its birth, and this is one of the reasons why we are glad to be back here again. There are other reasons, too, why I take personal satisfaction in being the President of the Section at this Dublin meeting, and one of them is that it was the first of my predecessors in the Regius Chair of Chemistry in the University of Glasgow, Dr. Thomas Thomson, who was the President of the Section at that first meeting in 1835.

Many of my predecessors in this Presidential chair have taken the opportunity to glance backward at the developments in some field of chemical science during a specified period. Others have given masterly surveys of new developments with which they have been especially associated. I propose to follow neither of these precedents but rather to survey a field in which precise scientific knowledge is largely lacking and the implications of what knowledge there is rather conjectural. I shall draw attention chiefly to experimental work which still remains to be done rather than to what has been accomplished. I make no apology for taking this line. The increase in cancer of the lung in recent years has been so marked as to be a cause for great concern. The subject is one of considerable topical interest and there are also vested interests involved. Largely because of the nature of the problem, the public has been given a rather confused picture of what are the real issues involved. I am not sure that I can do much to remedy this, but I can at any rate attempt to present to a largely scientific audience what seems to me to be the present position and the directions in which further enquiry must be pursued.

Professor Clemo made some reference to this subject in his Presidential Address to the Section at the Liverpool meeting in 1953. He suggested that the increase in respiratory cancer focuses attention on the smoke in the air we breathe and he gave an account of some work on the isolation of polycyclic hydrocarbons from the material collected in the air-filters of the new building for his department in Newcastle. Atmospheric pollution is not, however, the only factor which may be involved in the increase in lung cancer, and is probably not the major factor. Epidemiological studies have provided a weighty body of evidence which seems to incriminate tobacco smoking, particularly in the form of cigarettes. In fact, the statistical evidence of an association between lung cancer and smoking is so strong that if cigarette-smoking were a paid occupation it is more than likely that lung cancer among those engaged in the 'industry' would be made an industrial disease and its victims made eligible for compensation.

But, as I have said, there are vested interests. There is the tobacco industry, which is naturally reluctant to go into liquidation. There are Chancellors of the Exchequer who derive a considerable revenue from tax on tobacco. Above all, there is a large public which derives considerable satisfaction from smoking and is disposed to exhibit strong sales resistance to the vendor of the idea that its health is endangered by its smoking habits. If the onset of cancer could be seen to be a direct and immediate consequence of the act of smoking, the onlooker would be

[1] Address delivered at the Dublin Meeting of the British Association on September 5, 1957.

greatly impressed and would take heed of the warning. But we know from the nature of the disease that cancer is not like that. It comes about, often late in life, as a result of influences which may have operated during a period of many years and there is no obvious connection between these influences and the ultimate effects. Moreover, it is well known that there are many non-smokers who contract lung cancer and many heavy smokers who live to a considerable age without showing any sign of the disease. There was a recent report from America of a man who decided to celebrate his hundred and seventh birthday by giving up smoking, a habit in which he claimed to have indulged for just one hundred years. It is not altogether surprising that a public which does not want to be convinced is inclined to be sceptical of the implications of statistical arguments which it is in no position to evaluate. The average person, even if he accepts it as established that smoking is attended by the risk of developing lung cancer, is inclined to look upon this as a relatively minor risk on which he can take a chance. He is well accustomed, either from choice or by obligation, to adopt this attitude towards many of the other hazards to which he is exposed.

I have referred to statistical evidence of an association between tobacco smoking, particularly cigarette smoking, and the incidence of lung cancer. This evidence is, I think, so overwhelming that few would dispute that an association has been established. Where opinions do differ, and differ quite legitimately, is in the interpretation to be placed on this. Bradford Hill and Doll, who have been responsible for the principal statistical investigations in the United Kingdom, and Wynder, and also Hammond and Horn, who have conducted similar studies in the United States, are of the opinion that the results demonstrate that smoking is a principal cause of lung cancer.

This conclusion is not universally accepted and has, in fact, been vigorously challenged. Kotin, in California, in a comprehensive survey of the role of atmospheric pollution, has suggested that successful as the attempts have been to relate increasing incidence of lung cancer with increasing tobacco consumption, an even closer relationship is found with respect to motor-fuel consumption, to numbers of motor-vehicle registrations and to mileages on asphalted roads. His general conclusion is that increasing atmospheric pollution is a much more important factor than tobacco smoking in explaining the increase in lung cancer. A similar conclusion is reached by Hueper, of the United States Public Health Service, who is very critical of the arguments used to support the view that smoking is responsible for the increase in lung cancer. He takes the view that 'it would be most injudicious mainly to base the future preventive control of lung-cancer hazards on a theory of such doubtful scientific merits and to concentrate the immediate epidemiological and experimental efforts on this apparently overpropagandised concept'. According to Hueper, the evidence points much more strongly to the conclusion that lung cancers are environmental in character and have an occupational or industrial origin.

In support of this view that industrial factors are mainly responsible for the increasing incidence of lung cancer are the findings that there seems also to be an association with density of population, and in this connection it may be significant that the death-rate from lung cancer is higher in the United Kingdom than in any other country. Moreover, Eastcott has found that immigrants to New Zealand from the United Kingdom have a higher incidence of lung cancer than New Zealand-born persons of the same stock and he attributed this to the influence on the immigrants of their former environment, there being no difference in the tobacco consumption of the two groups. However, the protagonists of the smoking theory suggest that the difference lies in the fact that *cigarette* smoking is more prevalent in Britain than in New Zealand.

In the *Statistical Bulletin* for October 1954, it is reported that a considerably

higher death-rate from respiratory cancer was experienced by white men insured under industrial policies in the Metropolitan Life Insurance Company (for the most part urban wage-earners) than by white men in the general American population. On the other hand, ordinary policy-holders, drawn chiefly from middle and well-to-do groups, have had *lower* death rates from this cause than white males in the general population. This is held to indicate an association between respiratory cancer and factors related to environment, occupation and social class.

Dublin, in the period 1950–53, had mortality rates from lung cancer in males of the age-group 45–64 not significantly different from the rates for four English cities of comparable size; namely, Bristol, Leicester, Nottingham and Sheffield. But in 1954 the consumption of coal in Sheffield was more than ten times that in Dublin; and in 1938 the Sheffield Corporation Transport Commission alone burnt as much diesel oil as the whole of Eire. As the cigarette consumption of Dublin and of Sheffield seems to be about the same, this might indicate that smoking is a more important factor than atmospheric pollution. Against this conclusion is the fact that the lung-cancer rate in large American cities is much less than in English cities and comparable with that in English rural areas, although the consumption of tobacco per head is greater in America than it is in England.

There is thus a conflict of evidence, or at any rate a conflict of opinion on the interpretation of the evidence, and in considering what new kind of evidence might assist in arriving at a decision the experimental scientist is naturally attracted by the possibility of an experimental approach to the subject. It is true that Wynder has suggested that proof that smoking causes cancer in man can come only from human statistical and epidemiological studies and that the purpose of animal experimentation is rather to define the mechanism by which a specific agent causes cancer. But I think that most experimental scientists would find the conclusions of statistical studies more convincing if they could be backed up by supporting experimental evidence. It was certainly with this in mind that the Medical Research Council decided to set up two committees to promote and to co-ordinate experimental studies in Britain into the nature of tobacco smoke, with specific reference to its possible influence in producing lung cancer. I am the chairman of one of these committees and also honorary director of a new research group set up by the Medical Research Council to pursue chemical enquiries in this field, and I offer this as the main justification for my choice of subject for this address.

I should like first to allude briefly to the nature and extent of the statistical evidence of the association between smoking and lung cancer, although clearly this is not the occasion for detailed discussion of this evidence. Then I propose to outline the way in which some of the chemical implications of this statistical association are being investigated. Even if it is true that smoking is a major cause of lung cancer, or at any rate of certain types of lung cancer, it is clearly not the only cause, and in this connection it will be necessary to try to evaluate the influence of general atmospheric pollution. Finally, I shall refer to epidemiological studies which indicate that there are specific chemical industries in which workers are especially liable to contract lung cancer.

Statistical Studies

Mortality figures show that deaths from lung cancer have been increasing during the last half-century in all countries for which figures are available. The increase has been much greater in men than in women. In England and Wales, which have the highest rates, the number of recorded deaths from cancer of the lung has been more than doubled in ten years, and lung cancer is now responsible for one death in eighteen in males of all ages. The accompanying table, taken from an article by Dr. R. Doll in Volume III of *Advances in Cancer Research*, gives the death rate from cancer of the lung in a number of countries in 1950 or 1951. There is wide

variation among the figures for men, but there is some measure of uniformity for women (excluding the first three countries). This suggests that environmental factors which may lead to the disease in men do not influence women in the same way.

Crude Death-Rate from Lung Cancer per million

COUNTRY	YEAR	MEN	WOMEN	PERSONS
England and Wales	1951	530	91	303
Scotland	1951	470	104	279
Finland	1950	353	61	201
Holland	1951	271	45	158
Switzerland	1950	252	38	136
U.S.A.	1951	214	45	129
Denmark	1951	185	46	115
Australia	1951–2	173	37	106
Canada	1951	154	34	95
France	1950	161	47	87
Sweden	1951	111	43	77
Norway	1951	81	39	60
Iceland	1950	—	—	42*

* The amount of tobacco used as cigarettes in Iceland is said to be small.

Much of the increase which has been recorded is due to the rising average age of the population, but allowance can be made for this and there is still a large real increase if we adjust the figures to take into account this change in the age distribution. Some of the increase may also be attributed to improved diagnosis. It has, in fact, been suggested that the whole of the increase can be accounted for in these two ways, but this is not generally accepted and there are a number of reasons for believing that there has been a real absolute increase in the incidence of lung cancer.

Quite a number of investigations into the association with tobacco smoking have now been carried out, in several countries. About fifteen of them have been retrospective investigations—examining the past smoking habits of people who die from lung cancer and comparing them with those of a control group—but results of two important prospective enquiries have recently become available. In this method of approach enquiries about smoking habits are made in a selected population and a study is made of the causes of subsequent death. This is more objective than the retrospective method, in that it does not rely to the same extent on memories of past events and the person questioned is not influenced by the presence of a specific disease. Doll and Bradford Hill, in 1951, sent questionnaires to 59,600 men and women on the British Medical Register. Replies numbered 41,024, of which 40,701 were sufficiently complete to be utilised (34,494 from men and 6,207 from women). After four and a half years an analysis has been made of causes of deaths of 1,854 of these doctors. Of a total 1,714 deaths among men aged thirty five years or more, 82 were due to lung cancer. Among women of all ages there were only three deaths from lung cancer. The results showed a steady increase in the death-rate from lung cancer with increasing amount of tobacco smoked. The rate rises from 0·07 per 1,000 deaths in non-smokers[1] (but this figure was based on observations of one death only) to 0·47 per 1,000 in 'light' smokers of 1 to 14 grammes a day to 0·86 per 1,000 in 'medium' smokers of 14 to 24 grammes a day, and finally to 1·66 per 1,000 in smokers of 25 grammes or more a day. The death-rate of the heavy smokers is about twenty times the death-rate of non-smokers. The mortality from lung cancer was significantly greater in cigarette smokers than in pipe smokers. Deaths from cancer in other sites revealed, with one possible

[1] Doll and Hill define a non-smoker as a person who has never consistently smoked as much as 1 gramme of tobacco a day for as long as one year. One cigarette is equivalent to 1 gramme of tobacco.

exception, no association between mortality and smoking and no gradient related to the amount smoked.

Under the auspices of the American Cancer Society an even larger prospective investigation has been in progress since 1952. Usable questionnaires were obtained which gave the smoking characteristics of 187,766 white males between the ages of fifty and sixty-nine. After about twenty months, 4,854 deaths had occurred, of which 167 were attributed to lung cancer. In 1954, a preliminary report of this enquiry was published by Hammond and Horn, whose conclusions were very similar to those of Doll and Hill. Thus they stated: 'The best estimate that can be made at the present time is that lung-cancer deaths are from three to nine times as common among men with a history of cigarette-smoking as among men who have never smoked regularly and that lung-cancer deaths are from five to sixteen times as common among men who smoke one pack or more a day'.

A report covering the first four years of this enquiry and dealing with 11,870 deaths has now been published. Full details are not available at the time of writing, but according to the daily press the report shows that cigarette smokers are ten times more likely to die of lung cancer than non-smokers. There was said to be a far lower degree of association between death-rates and cigar-smoking.

I have perhaps, said sufficient to indicate the scale on which these investigations are being made and the conclusions which have been drawn from them, which are completely in line with those of the earlier retrospective investigations. To my nonstatistical mind the impressive feature of these conclusions is not so much that there is an association between smoking and lung cancer as that the death-rate from the disease seems to be proportional to the number of cigarettes smoked. If this is a genuine correlation, then it is difficult to conceive of any other explanation than that the smoking is a causative factor. It has, nevertheless, been suggested that the relationship is an indirect one, and that the same factors, psychological or otherwise, which are conducive to heavy smoking also lead to a disposition towards lung cancer. But this, surely, is special pleading. While it is not germane to the subject of my address, we should not overlook the possibility that smoking may have other hazards to health than an increased risk of developing lung cancer. The latest report of Hammond and Horn does, in fact, suggest that smoking increased the chance of fatal heart attacks by 70 per cent.

Before leaving the subject of these statistical studies, there is one other aspect of the matter to which I should like to refer. This relates to the histological type of tumour. There are several different forms of lung cancer. Those commonly found in laboratory animals, or induced in them by chemical carcinogenic agents, are of a glandular type known as adenocarcinomas. This type, although it occurs in man, is much less common than bronchial carcinoma of the squamous, oat-cell and anaplastic types. If one pays attention to the type of tumour, then one finds that adenocarcinomas occur to about the same extent in men and women and that this type of tumour is relatively more common among non-smokers with lung cancer than among smokers. Kreyberg, in particular, has studied the histology of lung tumours in Norway and has concluded that the increase that has taken place is largely confined to tumours of the other histological type (squamous, large-cell and small-cell carcinoma) and is related to a new carcinogenic situation that has arisen during the present century. As regards adenocarcinomas, Kreyberg concludes that in Norway the whole of the reported increase in female cases as well as part of the increase in male cases is due to better diagnostic facilities. The evidence from this and other sources, while not extensive, tends to support the view that the two different types of tumour are due to different causes and that only one of them (squamous, oat-celled and anaplastic type) is due to the factor, whether smoking or some environmental factor, which has led to such a large increase in incidence of lung cancer in men.

Experimental Investigation of Tobacco Smoke

If we accept the view that the numerous statistical studies which have been carried out point to smoking being a principal cause of lung cancer, then presumably the tobacco smoke contains some ingredient which has a carcinogenic effect on lung tissue. It ought to be possible, therefore, to obtain some experimental evidence of this, and it is this assumption which underlies many laboratory investigations now in progress. Tobacco smoke is an aerosol of which the particulate phase consists of oily droplets. These are said to range in size from $0 \cdot 3$ to $1 \cdot 0\mu$ at the time of formation, but the median size increases rapidly. The particle size is important, as it is upon this that the entry and retention in the lungs and bronchi depend. By electrostatic precipitation or absorption in a solvent, or by strong cooling, the smoke is condensed to a light-brown tar which at ordinary temperature rapidly becomes dark brown.

With a tar prepared by smoking cigarettes in machines so constructed as to attempt to reproduce the conditions of human smoking, Wynder has obtained malignant skin tumours in mice. The tar was applied repeatedly to large areas of shaved skin during a long period and cancers were obtained in a considerable proportion of the treated animals in several strains of mice. Skin tumours were also produced in rabbits by painting with tobacco tar, for more than five years. The tar is evidently a very weak carcinogen, as if it was diluted two or three times or the painting stopped after a short time, skin tumours in mice were not obtained. Other workers have been less successful in obtaining cancers with tobacco tar, although there have been reports of induction of skin tumours in mice and rabbits, mostly non-malignant and usually by tars formed by destructive distillation of tobacco. Wynder and Wright are attempting to concentrate the active constituent of the tar and have reported that it is contained in the neutral fraction. The presence of the known carcinogen, 3 : 4-benzpyrene, in tobacco smoke, has been demonstrated by Cooper, Lindsey and Waller, but according to Wynder the amount of this is not more than 2 parts per million, an amount insufficient to account for the tumours obtained. Kosak obtained tumours with a cigarette tar fraction in which benzpyrene could not be detected. The inference is, therefore, that the tar contains other constituents which are carcinogenic to mouse skin. Wynder has estimated that the activity of the tobacco tar approximates to that of a $0 \cdot 005$ per cent solution of 3 : 4-benzpyrene.

The presence in tobacco tar of such a potent carcinogen as 3 : 4-benzpyrene, known also as a constituent of coal-tar, naturally leads to speculation as to whether it may be responsible for lung cancer in smokers. It was detected and estimated by Lindsey and his collaborators by ultraviolet absorption spectroscopy, so that the identification is perhaps less certain than if it had been isolated. The amount which they estimated is of the order of 1 microgramme in the smoke from 100 cigarettes, although other workers have reported more than ten times this quantity. Whether benzpyrene would be carcinogenic in such low concentration is problematical. It has been claimed that its formation is suppressed in cigarettes made by using paper which contains ammonium sulphamate. In any event, it has not yet proved possible to produce bronchial carcinoma of the type observed in man by the application of 3 : 4-benzpyrene or similar carcinogens to experimental animals, nor has any success been achieved in attempting to produce such tumours with cigarette smoke. A satisfactory method of biological assay of the smoke has yet to be found, and this may well be the limiting factor which will impede progress on the chemical investigation of the smoke. Some recent experiments by Lasnitzki on the effect of carcinogens on foetal lung tissue cultured *in vitro* seem to show promise of development in this direction.

Intriguing questions are raised by the epidemiological finding that pipe smokers are less liable to lung cancer than cigarette smokers and that cigar smokers are

almost immune. Are there differences in the smoke to account for this, and, if so, what are they? Or is it that only the cigarette smokers inhale the smoke? The paper in which cigarette tobacco is wrapped has also been considered as a source of carcinogens. Cigarettes composed entirely of cigarette paper have been used in smoking-machines and have given tars in which 3:4-benzpyrene has been shown to be present. One estimate is that the cigarette paper accounts for about a third of the benzpyrene formed by smoking a cigarette. Other estimates put the amount at less than this. One very striking difference between cigarette smoke and cigar smoke is that cigarette smoke is acidic in reaction, whereas cigar smoke is strongly alkaline. Smoke from pipe tobacco is said to vary in reaction. These differences are thought to be related to the methods used in curing the tobacco. Whether the presumed carcinogenicity of cigarette smoke is related to its acidity is a question that merits more detailed consideration than it seems to have received.[1]

Composition of Tobacco Smoke

Tobacco smoke might seem to be an unpromising material for chemical study, yet in spite of this a large number of individual constituents has been recognised. These include hydrocarbons, both aliphatic and aromatic, alcohols and phenols, aldehydes and ketones, acids, alkaloids and other nitrogen compounds, as well as a number of inorganic constituents. Not all of these have been identified with equal certainty; and some of them undoubtedly arise from additives, which in some countries are incorporated in tobacco. The chapter by Kosak in *The Biologic Effects of Tobacco*, edited by Wynder, lists about a hundred different substances which have been reported to be present in tobacco smoke and the list is certainly not exhaustive.

Considering the drastic treatment to which tobacco is subjected when converted into smoke, the range of substances which the smoke contains is surprising, as is the number of compounds which may survive either without change or with relatively little change. This emphasises the need to extend our knowledge of unburnt tobacco, both green tobacco and cured tobacco, and attention is being given to this. Among the paraffins present in smoke is *n*-hentriacontane, present also in tobacco itself, and it has been suggested that much of the polycyclic aromatic hydrocarbons, including 3:4-benzpyrene, of tobacco smoke is derived from this. Wynder and Wright, and also Lam, have shown that a mixture of polycyclic aromatic hydrocarbons similar to that present in smoke is produced from hentriacontane by cracking. This has led to the suggestion by Wynder that the removal from tobacco of paraffins of this type, by washing with solvents, may reduce the polycyclic aromatic hydrocarbon content of the smoke and thus lessen the risks attending smoking.

The pentaterpenoid alcohol, solanesol, has been found in both green and cured tobacco by Rowland, Latimer and Giles, who assigned to it the following structure:

$$Me_2C{=}CH{-}CH_2(CH_2.\overset{\overset{Me}{|}}{C}{=}CH.CH_2)_8.CH_2.\overset{\overset{Me}{|}}{C}{=}CH.CH_2OH.$$

More recently, solanesol has been isolated from tobacco smoke by Mold and Booth. It is remarkable that such a compound should survive. The alkaloids of tobacco smoke tend to be more highly aromatic compounds derived from the tobacco alkaloids, and formed from them by pyrolytic degradation. They have been very incompletely investigated and merit further study.

[1] Dr. W. Carruthers, of the M.R.C. Carcinogenic Substances Research Group, has confirmed an observation reported earlier that although the mainstream smoke of a cigarette is acid, the sidestream smoke is alkaline in reaction.

Until quite recently, the only polycyclic aromatic hydrocarbon of pronounced carcinogenic activity shown to be present in tobacco smoke was 3 : 4-benzpyrene. To this has now been added 3 : 4—9 : 10-dibenzpyrene, shown by Bonnet and Neukomm by combined chromatographic and spectroscopic techniques to be present in cigarette smoke and found by Lacassagne and his collaborators to be strongly carcinogenic. This finding, if confirmed, reinforces the evidence incriminating the neutral fraction of tobacco tar. It has been found that pipe smoking (of both pipe tobacco and cigarette tobacco) gives higher yields of polycyclic aromatic hydrocarbons than cigarette smoking. Moreover, Wynder found that tar from tobacco smoked in pipes gave twice as many tumours of the skin in mice as condensate from cigarette smoke. These two findings may be related to the fact, noted by Gilbert and Lindsey, that the temperature of burning tobacco in pipes (450–500°C.) is about 200–250°C. below that of cigarettes.[1] These findings do not readily accommodate the higher incidence of lung cancer in cigarette smokers as compared with pipe smokers, and it seems clear that the formation of complex carcinogenic hydrocarbons is not the whole story.

Current interest in the subject has stimulated new investigations of known types of constituents of tobacco smoke. Combinations of chromatography with spectroscopic examination have mostly been used. Chromatography of 2 : 4-dinitrophenylhydrazones has led to the detection of formaldehyde, acetaldehyde, propionaldehyde butyraldehyde, crotonaldehyde, benzaldehyde, acetone, methyl ethyl ketone and diethyl ketone in cigarette smoke. By paper chromatography of derived azo compounds, Rayburn and co-workers identified phenol, guaiacol and *o*- and *m*-cresol; while more recently Commins and Lindsey have identified, after methylation, the methyl ethers of *p*-cresol, the two naphthols, catechol, resorcinol, and quinol, all in cigarette smoke. In the range of acids, Hobbs and his collaborators have detected formic, acetic, propionic, butyric, *iso*butyric, valeric, *iso*valeric, caproic, heptylic, caprylic, nonylic, capric, benzoic, glutamic and nicotinic acids, and the amides of the last two of these.

Further studies on these lines must aim at the careful control of the conditions of smoking, with attention to temperatures of combustion, using tobacco of exactly defined origin and avoidance as far as possible of secondary changes in the tar. In the separation of constituents some of the newer techniques such as vapour phase chromatography, at any rate in the lower boiling ranges, and the use of the Craig counter-current apparatus may be expected to help. Pure components will need to be submitted to systematic biological test, and, as I have suggested already, the proper test procedure has yet to be devised. Our biological collaborators are wrestling with this problem.

In addition to the organic constituents to which I have referred, tobacco smoke also contains a range of inorganic compounds, mostly in very small amount. These should not be overlooked in any comprehensive study of the problem. Radioactive substances should certainly be regarded as suspect and from this point of view potassium has received attention. It has been reported that the average radioactivity of one cigarette is approximately that of 25 milligrammes of potassium and that the radioactivity of the smoke from one cigarette is about that of 6 microgrammes of potassium, so that the remainder presumably remains in the ash. Such a low level of radioactivity in the smoke is hardly likely to be injurious. A more serious possibility lies in the arsenic content of the smoke and some attention has been devoted to this. Daff and Kennaway found wide variation in the arsenic content of fifteen brands of cigarettes. A high content may result from the use of arsenical sprays and dusts used as insecticides for the tobacco plants. As there is

[1] The suction combustion temperature of cigarettes has been given as 700° C., although this must be subject to considerable variation and will be influenced by such factors as humidity and tightness of packing.

evidence that arsenic may be responsible for certain occupational skin cancers in man, this has naturally come under suspicion. But Turkish tobacco is low in arsenic content, whereas there is a high incidence of lung cancer in Istanbul where Turkish tobacco is smoked almost exclusively, and this has been cited as evidence against the view that arsenic in tobacco smoke is responsible for lung cancer.

Atmospheric Pollution

I come now to the question of atmospheric pollution, which has been the subject of extensive surveys. It is now generally agreed that there is a higher incidence of cancer of the lung in inhabitants of towns and urban districts than in inhabitants of rural areas. Some statistical studies have shown a significant correlation between density of population and of industrialisation and the incidence of lung cancer. This has been variously attributed to better facilities for diagnosis in the towns, classification by place of death rather than by residence (the large hospitals are mostly in the towns), differences in smoking habits, and atmospheric differences. All these factors may play a part, although it is probable that the availability of diagnostic facilities and differences in smoking habits as between town and country are less important now than they have been in the past. It must, of course, be remembered that there is a considerable time lag between the first exposure to a carcinogenic environment and the appearance of tumours and that the cancers which are now causing deaths may be due to influences which began to operate twenty or thirty years ago, or even longer.

Stocks and Campbell, from a study of lung-cancer death-rates among different categories of smokers in rural, semi-rural and urban environments, in Liverpool, Cheshire and North Wales, concluded that tobacco smoking and atmospheric pollution play a joint role in the development of lung cancer. They found that the urban/rural ratio was about 9 : 1 among non-smokers[1] and gradually fell to a figure near unity in heavy smokers, but the absolute urban excess was the same in all categories of smokers. They also reported a close correlation between lung-cancer death-rates and the total estimated amount of 3 : 4-benzpyrene inspired, both from the atmosphere and from tobacco smoke, and this led them to suggest benzpyrene as the sole agent responsible. This, however, is probably an undue simplification. Although 3 : 4-benzpyrene, as a constitutent of coal-tar, is occluded in the soot which pollutes the atmosphere—Goulden and Tipler estimated about 300 milligrammes of benzpyrene per kilogramme of soot—there is evidence from independent sources that coal-tar contains other carcinogens. Potent carcinogenic tar fractions have been obtained which appear to be free from benzpyrene. Soot is deposited in the lungs of town-dwellers, and it has been suggested by Cooper that it retains the benzpyrene in a firmly bound innocuous state and that the role of tobacco smoke, which contains powerful solvents, is to elute the benzpyrene and so bring it into intimate contact with the lung tissue. This is an interesting hypothesis, but if it were true one would expect the urban/rural ratio among smokers to be higher than it is.

Even in the absence of visible smoke, benzpyrene has been shown to be present in the atmosphere in amounts of the order of 3 microgrammes per 100 cubic metres of air. The concentration rose sharply during the winter, according to the findings of Goulden and Tipler, in Britain.

The other main sources of atmospheric contamination which have been suspect as causes of lung cancer are the exhaust fumes from internal-combustion engines and diesel engines. Kotin and his collaborators in Los Angeles detected in the exhaust fumes of an internal-combustion engine a number of polycyclic aromatic hydrocarbons, including 3 : 4-benzpyrene. Under conditions of maximum hydrocarbon production (acceleration from low speed), benzpyrene was formed at the

[1] By contrast, Doll found no significant difference in the lung-cancer mortality-rate among non-smokers living in London, 'other urban areas' and 'rural areas'.

rate of nearly 1 milligramme per minute and benzene extracts of the particulate phase gave malignant skin tumours in mice. Diesel-engine exhausts have also been shown to contain benzpyrene under conditions of inefficient operation.

Kotin, Falk and Thomas also obtained tumours on mouse skin with the particulate phase of artificially produced 'smog' free from aromatic hydrocarbons. This was prepared by treatment of petrol vapours with ozone and is said to resemble authentic smog closely. Mice exposed to the synthetic smog had a greater incidence of lung tumours (adenomas) than the controls. The smog was thought to contain epoxides and other oxidation products of aliphatic hydrocarbons, and in a synthetic approach butadiene diepoxide and 1:3-*bis*-(2:3-epoxypropoxy) benzene was found carcinogenic to both rats and mice. All this opens up important avenues of chemical investigation.

The dust from tarred roads has also been suspected of being implicated in the increased incidence of lung cancer. Road tar is carcinogenic to mouse skin, and carcinogenic substances may also be present in the carbon black incorporated into motor tyres. There is no concrete evidence to incriminate these materials, but the possibility remains.

Industrial Lung Cancer

Finally, I must refer briefly to the enhanced liability to lung cancer to which workers in certain industries are exposed. There can be little doubt that specific chemical substances are responsible and that the tumours represent an occupational hazard. In the limited time that remains I can do no more than summarise the main conclusions. The classical example of this is the occupational disease of the miners of Schneeberg in Saxony, and Joachimsthal (Jáchymov) in Bohemia, which has been known for at least five hundred years, although it was less than a hundred years ago (1879) that it was diagnosed as primary cancer of the lung. An interesting historical account is given by C. V. Weller in *Causal Factors in Cancer of the Lung*, published in 1956. The incidence of the disease has been between 75 and 80 per cent in the Schneeberg miners and between 40 and 50 per cent in the Joachimsthal miners. It is almost certainly due to ionising radiations from radioactive gases and/or dust. Experimental confirmatory evidence was provided by Lisco and Finkel, who, in 1949, reported the production of bronchial carcinoma in the lungs of rats exposed to an aerosol containing radioactive cerium. It is to be hoped that due regard is being paid to this hazard in the much more extensive uranium and thorium mining now being undertaken and in the handling of these materials. Increased liability to lung cancer must also be regarded as a definite hazard associated with radioactive fall-out from nuclear and thermonuclear weapons, although, of course, not the only hazard.

It is possible that atmospheric radioactivity arising from the natural decay of radioactive minerals is responsible for some of the lung cancer which now occurs,[1] although clearly the large rise in the incidence during recent years cannot be attributed to this cause. In order to assess the importance of this factor one needs to know whether there is a threshold dose of radiation below which exposure to radioactivity is innocuous. The same sort of consideration also applies in regard to carcinogenic chemicals such as 3:4-benzpyrene.

This occupational lung cancer among miners of the Ore Mountains of Saxony and Bohemia represents, fortunately, something quite exceptional. Other examples of occupational lung cancer are less spectacular, but nevertheless an impressive body of evidence is building up. The causative agents are in some cases metals or their salts. Thus, excessive numbers of lung cancer cases have been reported among nickel workers and workers in the chromate industries and also gas-retort workers.

[1] In this connection attention has been drawn to the radium content of chimney smoke and also to the increased radon concentration in the atmosphere in smog conditions.

More recently, Faulds and Stewart have observed an increase among haematite miners in Cumberland. Beryllium is retained for a very long period in the human body and gives rise to curious histological changes in the lungs, although these have not been proved to be pre-cancerous. Nor have lesions associated with silicosis. One of the few reported instances of the successful experimental production of bronchial carcinoma was recorded by Vorwald who observed such tumours in the lungs of rats which had, for more than a year, inhaled a dust of beryllium salts. Other industrial materials which have been implicated in occupational lung cancers are asbestos, arsenical dust and, less convincingly, crude isopropanol, mustard gas, and petroleum products. The total number of cases of respiratory cancer attributable to all these occupational causes may, indeed, be exceedingly large. In fact, Hueper regards such environmental causes as far outweighing tobacco smoking in importance.

Conclusion

I have ranged over a very wide field and I have had in consequence to be very superficial. I have done this quite deliberately, in preference to discussing in detail some particular aspect of the problem, in order to emphasise that the whole question of lung cancer and its alarming increase is very complex and is unlikely to admit of any simple solution. The balance of evidence is strongly in favour of the view that carcinogenic chemical agents are primarily concerned, although in the present state of our knowledge it is very difficult to arrange the sources of these in any undoubted order of importance. I am convinced that the chemist must play an increasing part in the study of the various factors involved. Large dividends are unlikely to accrue rapidly, but it is to be hoped that increasing attention will be given to a subject of such wide human interest. The ultimate rewards of a successful attack will be quite considerable.

I am greatly indebted to Dr. W. Carruthers for valuable help in the survey of the literature which the preparation of this address has entailed.

ORDOVICIAN VOLCANOES[1]

ADDRESS BY DR. G. H. MITCHELL, F.R.S.

PRESIDENT OF SECTION C

WHEN last the Association met in Dublin, forty-nine years ago, our President, that distinguished Irishman John Joly, the centenary of whose birth we celebrate this year, addressed the section on 'Uranium and Geology'.

One of Joly's teachings was that a direct connection exists between the heat accumulated within the crust of the earth as a result of radio-activity and the periods of earth-movement and the outbursts of vulcanicity which recur from time to time.

Today I wish briefly to consider one of those violent outbursts of volcanic activity. It took place in Ordovician times, according to some authorities about 350 to 400 million years ago. The lavas and tuffs erupted in those far-off days are now greatly folded, faulted, cleaved and altered, but they form at this moment of geological time some of the most delightful scenery of the British Islands.

Ordovician volcanic rocks are found in Ireland near Dublin, at Balbriggan, Kildare and Lambay Island. A considerable belt extends southwards from Arklow Head almost to Dungarvan in Waterford. North of Dublin are the outcrops of Collon and Tyrone, while to the west are those of Lough Mask.

Across the Irish Sea, Ordovician rocks of Wales and Shropshire, occupying wide tracts of largely mountainous country between the Menai Straits and the Bristol Channel, contain much volcanic material, particularly in Caernarvon, Merioneth and Pembrokeshire.

In north-western England lies the Lake District with an outcrop of Ordovician volcanic strata well over 250 square miles in area. Still farther north, along the margin of the Southern Uplands from Girvan to the Lammermuir Hills, volcanic rocks occur within the main belt of the Ordovician of Scotland. Smaller inliers are found among the Silurian rocks of southern Scotland and there are scattered exposures as far afield as Arran and along the Highland Boundary Fault near Aberfoyle and Stonehaven.

At the other extremity of Great Britain the Meneage Quartzite in Cornwall is considered to be Ordovician, but it is uncertain whether the neighbouring spilites are of that age.

The present distribution of Ordovician rocks at outcrop is the result of the varying geological processes which have affected them during the vast period of time which has elapsed since their deposition. Doubtless other strata of the same age either lie hidden beneath later rocks or have been completely removed by denudation during the periods of uplift and erosion which from time to time have occurred as the great Geological Cycle of uplift, denudation, submergence and sedimentation has been repeated.

The Lower Palaeozoic strata of Britain were deposited during a long period which followed the severe earth-movements at the end of the Pre-Cambrian and continued until terminated by the mountain-building movements often known as the Devonian orogeny. The magnitude of these two sets of movements and the great erosion which accompanied them is now revealed by the flagrant unconformities below Cambrian strata and at various levels between the higher Silurian beds and the lowest Carboniferous rocks. The deposition of Lower Palaeozoic times was punctuated, however, by other movements which can be deduced from the presence

[1] Address delivered at the Dublin Meeting of the British Association on September 9, 1957.

of lesser unconformities, some of which appear to be of greater importance than has previously been thought.

Following such authorities as L. J. Wills and O. T. Jones it is possible to recognise a great Welsh Lower Palaeozoic Geosyncline extending in a south-westerly direction through the centre of Wales. For part of the Ordovician Period at least, this was probably separated from a similar Irish-Scottish Basin—the Moffat Geosyncline—by a land-mass which was elongated north-eastwards and covered the area of the present Lake District, perhaps extending at times into the latitudes of Wales and Scotland.

The thickness of the deposits formed in Lower Palaeozoic times was enormous; in Wales it amounted to some seven miles of sediments and volcanic rocks. Thus a very considerable sinking of the earth's crust was necessary to accommodate this vast pile. It is not surprising that on occasion an opposite reaction was set up and volcanic materials were erupted instead!

No contemporaneous volcanic rocks are recorded with certainty in the Cambrian of Britain, but volcanic activity flared up in the Ordovician and eruptions, often of great violence, recurred throughout that period. The greatest intensity was probably reached about the middle of Ordovician times and thereafter they died down, though minor outbursts persisted even into the Silurian Era.

In order to restrict this address to a reasonable compass I have confined myself to a consideration of the evidence of actual extrusive activity, ignoring the many hyperbyssal and plutonic rocks which belong to the Ordovician vulcanicity. This summary would have been quite impossible without the work of a host of fellow-workers in our science; to them I return grateful thanks.

The nature of the Ordovician volcanic rocks suggests that, whether the vents from which they came were subaerial or submarine, the majority were deposited under water. Many of the tuffs contain an appreciable amount of ordinary sediment and, like the lavas, are often interbedded with true sediments. The lavas frequently display pillow structure, by itself generally held to be evidence of solidification under water. It is only in a few cases such as the beds of Rhobell Fawr and the Borrowdale Volcanic Series that there would seem to have been an appreciable thickness of subaerial tuffs and lavas and even in the latter case some workers have claimed that they also were entirely submarine. Throughout these two groups of great thickness there is practically no interbedded sedimentary material.

Little is known of the actual vents from which the rocks were erupted—a marked contrast for example to information available regarding Carboniferous volcanoes. Frequently it is possible, by considering their thickness and distribution, to suggest the direction from which the deposits we now see were derived. Indeed it is clear that in some areas there were numerous vents and that the eruptions were not fissure eruptions, but though subsidiary vents are recorded in places, the main orifices are presumably either concealed by later rocks or are no longer recognisable on account of erosion. The character of the pyroclastic rocks, ranging from coarse agglomerates to the finest dusts which must have spread for miles from their source, suggests Vesuvian eruptions. They include lithic, crystal and vitric tuffs, as well as welded tuffs which probably originated in Peléan explosions.

One of the characteristic features of Ordovician lavas is the frequency of flow-brecciation. Though well-known in modern lavas, such structures have often in the past been recorded as coarse pyroclastic rocks. This has led to considerable confusion in the mapping and a false estimation of the proportions of lavas and pyroclastic deposits.

The composition of both tuffs and lavas ranges from andesites to rhyolites but, though basalts are rare, there is a notable development of spilites. The eruptions have therefore been referred to the Pacific and Spilitic suites. Individual lava-flows are generally restricted in their extent and vary greatly in thickness. Neighbouring

vents were often apparently producing andesitic and rhyolitic lavas at approximately the same time.

The Ordovician volcanic rocks are now greatly altered and a high proportion of such secondary minerals as albite, chlorite, calcite, epidote, sericite and sphene make both the original composition of the phenocrysts and groundmass of the lavas and the crystals and lithic fragments of the tuffs difficult to determine. There is considerable evidence that much of the alteration was paulopost and in particular there was appreciable albitisation. The presence of both Spilitic and Pacific suites, which are held by some to be typical respectively of areas which suffered long and continued subsidence and of regions of lateral compression, suggests that both these conditions obtained at various times during the Ordovician.

Because of the great variations in thickness of the volcanic rocks from place to place there are many difficulties of correlation quite apart from those caused by the differing facies of the sedimentary members of the succession. As detailed mapping continues local unconformities are revealed, affording evidence of uplift, folding and erosion. In particular there is an important mid-Ordovician unconformity, pre-Caradoc in some places, perhaps pre-Llandeilo in others.

Arenig

Volcanic eruptions broke out early in the Arenig Period. A considerable volcanic field was situated in the far west of Ireland near Lough Mask. The main centre, however, was probably in Merioneth and though deposition extended from Fairbourne on the south-west by way of Dolgelley to Arenig on the north-east it did not reach Shropshire. There were, however, active volcanoes near the Lleyn Peninsula, though not even the finest showers of dust from these appear to have fallen in the Deudreth district of Merioneth or in Anglesey.

Several centres of eruption are suggested in South Wales for volcanic deposits are known from Trefgarn, near Haverford West, near Carmarthen, in the Prescelly Hills and on Skomer Island, though not at St. David's itself.

The Arenig rocks of the Lake District do not appear to include volcanic material, but across the Border at Ballantrae in Ayrshire there must have been violent eruptions. This and other centres presumably provided the lavas and tuffs which are now found in Arran, at various places in the Southern Uplands and as far north as the Highland Border at Aberfoyle and Stonehaven.

Ireland

On the western margin of the British Isles in County Mayo, west of Lough Mask, near the River Glensaul and Tourmakeady, the Mount Partry Beds, of *Didymograptus extensus* age, contain coarse tuffs and gritty tuffs upwards of 110 ft. thick. The coarse tuff is composed of angular blocks of felsite and the finer tuffs include lapilli of both felsitic and andesitic rock, but no lava-flows were reported by Gardiner and Reynolds.

In the neighbouring area of the Kilbride Peninsula to the south the same authors suggested that some coarse felsitic and spilitic breccias may be on the sites of vents. Here the Mount Partry Beds also include flows of much altered spilite hundreds of feet thick displaying flow-brecciation as well as pillow structure. The spilites persist into the Lough Nafooey district where, however, the tuffs are much thinner.

The early Arenig vulcanicity was prolonged into *D. hirundo* times and even perhaps later. Thus the Shangort Beds of Tourmakeady include rhyolite flows at the base, noted by Gardiner and Reynolds, who also showed that the Tourmakeady Beds of Glensaul, which lie above the Mount Partry Beds, contain tuffs and breccias totalling over 1,600 ft. The coarse tuffs are mainly composed of fragments of felsite, but include a breccia with limestone in addition.

The deposition of volcanic material did not extend to Killary Harbour some miles to the west, where rocks of middle and upper Arenig age contain no sign of lava or tuff.

A felsite near Charlestown accompanies tuffs and agglomerates. Cherty shales interbedded with the volcanic rocks have yielded to W. A. Cummins extensiform *Didymograptus* and other graptolites indicating an Arenig age.

Pillow lavas, also associated with chert, occur near Longford and may be Ordovician in age.

Wales

The Trefgarn Andesites of Pembrokeshire, described by Thomas and Cox, rest on *Lingula* Flags, and are overlapped within a short distance by the *Tetragraptus* Beds. They consist of alternating lavas and tuffs, with volcanic conglomerates including blocks of andesite, set in a pyroclastic matrix. The lavas are albite-pyroxene-andesites and the suggested thickness of the group as a whole is 750 ft.

The Llandeloy Ashes occur below the *Tetragraptus* Beds in the eastern part of the St. David's Peninsula; according to T. G. Williams they range from ashy sandstones to lithic and crystal tuffs with trachytic and andesitic fragments. Certain keratophyres in the *Tetragraptus* Shales may be lavas, but no flows are associated with the tuffs described above.

Within the *Tetragraptus* Shales at Abercastle there are some 250 ft. of keratophyres; Cox believed that as they flowed on the sea-floor they burrowed into the shales.

In the neighbourhood of Llangynog, near Carmarthen, Cantrill and Thomas formed the opinion that a sequence of augite-andesites, succeeded by perlitic and spherulitic rhyolites, which in turn are overlain by augite- and hornblende-andesites, was extruded during the formation of the lower *Tetragraptus* Beds. Accompanying the lavas are fragmental andesitic rocks, frequently mixed with sediment, but including pumice tuffs. The description of certain breccias suggests that they are flow-brecciated lavas.

A 50-ft. bed of ash occurs low down in the *Tetragraptus* Beds two miles north of Whitland.

Though there is no direct evidence, Thomas was inclined to assign the Skomer Volcanic Series of Skomer Island to the same general age as the Trefgarn rocks and he considered them to belong to the same petrographical province. The Skomer rocks are more varied, however, and include soda-rich and lime-poor lavas, considerably albitised, which were described as soda-rhyolites and trachytes, keratophyres, skomerites, marloesites, mugearites and olivine-basalts. They reach a total thickness of more than 2,900 ft. and were thought to be mainly subaerial, though they include remarkably few pyroclastic beds.

Probably somewhat later in age but still apparently within the Arenig, though perhaps continuing into the Llanvirn, is the Sealyham Group. As described by Thomas and Cox near Trefgarn and by W. D. Evans in the Prescelly Hills, the products of this fresh outburst of vulcanism, separated from the Trefgarn Andesites by shales containing *Didymograptus extensus*, took the form of a thick flow or flows of keratophyre. The lavas are brecciated and contain muddy material evidently caught up in the flows which were therefore presumably submarine. Thin tuffs follow and are succeeded by slates, above which comes another group of tuffs and some keratophyre flows, the whole being not less than 600–700 ft. thick. According to Evans the beds thin to between 100–250 ft. in the Prescelly Hills, where flow-brecciated and spherulitic soda-rhyolites with perlitic structure, soda-trachytes, keratophyres and tuffs occur.

The products of the earliest known Ordovician outbreaks are to be found at Rhobell Fawr, about six miles north-east of Dolgelley. Here A. K. Wells has shown

that a thick volcanic group lies with marked unconformity upon Tremadoc rocks and is itself unconformably overlain by a grit on which rest beds of *Didymograptus extensus* Zone age. It is evident that these volcanic rocks must have totalled many hundreds, if not thousands, of feet. They were presumably deposited rapidly and Wells even suggested that they were the products of a single great eruption. The lavas and explosion tuffs, both coarse and fine, which form this great pile, are believed to have been part of a subaerial cone. They contrast strongly with the majority of the volcanic rocks of Wales in that they are andesitic, but find their counterparts in the Trefgarn rocks of Pembrokeshire.

In the upper part of the Arenig the centre of volcanic activity seems to have shifted somewhat. From Dolgelley, Cader Idris and Fairbourne, Cox, Wells and B. Jones have described the Lower Acid or Mynydd y Gadr Group, which reaches 1,000 ft. in thickness on Cader Idris and in the Fairbourne country. On Cader Idris it consists entirely of rhyolitic lavas and tuffs which are the most acid of the Ordovician volcanic rocks in the area. In a general way tuffs form the lowest and highest parts of the sequence with nodular and compact rhyolites between, though even they are in places parted by tuff bands. Some lavas thin out westwards so that near Arthog tuffs predominate and include very fine-grained beds with conchoidal fracture known as chinastones.

At Beacon Hill, in the Fairbourne district, lava-flows re-appear and the group again attains a thickness of 1,000 ft. This led B. Jones to suggest that the Arenig volcanic rocks of this horizon were derived from more than one centre. Lower and upper volcanic divisions consisting of rhyolites and acid tuffs are separated by slates. Towards Fairbourne itself attenuation of lavas again occurs and the Lower Acid Group is composed of tuffs, many of which are of the chinastone type, evidently betokening increasing distance from the seat of eruption.

On Rhobell Fawr the succession commences with acid vitroclastic tuffs. A massive rhyolitic rock follows, the description of which suggests that it may be a welded tuff. It is overlain by basic agglomerates and well-bedded ashes, above which is a spilite flow with pillow structure.

Unless represented by the *Calymene* Ash the products of these eruptions did not reach the Arenig country.

Volcanic activity broke out in the Lleyn Peninsula in *Didymograptus extensus* Zone times when small outbursts of pyroclastic material formed ashy beds in the shales and accompanying sandstones. Activity increased somewhat in the succeeding two haemerae, during which the Rhiw Volcanic Group was erupted. These beds were stated by Matley to comprise acid tuffs, basic and rhyolitic lavas and spilites, the last-named with pillow structure. The order of extrusion was albite-basalt, augite-andesite, rhyolite and spilite. The rocks include crystal tuffs as well as a metamorphosed tuff, now forming a manganese ore. Near Pwllheli Matley found beds of ash in the *D. extensus-hirundo* Beds and also in the *D. bifidus* Beds. Like their counterparts at Rhiw these pyroclastic beds represent ash blown from volcanoes in other areas.

Scotland

Eruptions broke out in Arenig times near Ballantrae in Ayrshire, where now over 700 ft. of spilitic lavas, breccias and tuff beds crop out and are succeeded by some 700 ft. of coarse agglomerates and tuffs. Near the top of the lower group and within the upper are shales with graptolites indicating that they belong to the Zone of *D. extensus*. As noted by Peach and Horne many of the lavas display pillow structure and are associated with radiolarian cherts, shale and limestone, which fill the spaces between the pillows. The tuffs comprise crystals, crystal fragments and lapilli, some, but not all, comparable with the lavas. The coarseness of certain agglomerates suggests that the present outcrops are not far removed from the

original vents, but the variety of the tuffs has been thought to indicate derivation from separate orifices.

That volcanic activity was widespread in Scotland at this time is suggested by the fact that similar lavas, agglomerates and tuffs, sometimes associated with cherts, are found in the cores of sharply folded anticlines along a belt from Dalmellington as far to the north-east as Lamancha on the south-western border of the Midlothian Coalfield and in similar anticlines to the north-west of Newton Stewart. Bailey and McCallien have suggested that there may be two horizons of chert, separated by a considerable thickness of volcanic rocks and that at Raven Gill, near Abington, at any rate, the lavas are considerably younger than at Bennane Head near Ballantrae.

The Highland Border rocks near Aberfoyle include highly altered spilites, believed by Jehu and Campbell to be either Upper Cambrian or passage beds between Cambrian and Ordovician.

In North Glen Sannox, Arran, Gunn mapped lavas, tuffs and agglomerates, all much altered and apparently part of the metamorphic rocks of the Highlands. The lavas, which now appear as epidiorites, are recognised at two horizons, the lower marked by pillow structure. The similarity of these rocks to those of Aberfoyle and Ballantrae led to the suggestion by Tyrrell, J. G. C. Anderson and Pringle that they are of Arenig age.

Llanvirn

In Llanvirn times volcanic activity intensified greatly in Wales and extended from the Lleyn Peninsula to Shropshire and southwards to Builth, Llandeilo and to the south-western limit of the Ordovician outcrop at St. David's. There is no evidence of the continued eruption of the Irish craters of Lough Mask and activity in Scotland was suspended or very much reduced. At this time, however, the most violent of the known eruptions of the whole Ordovician Period broke out in the Lake District and left behind one of the most impressive piles of volcanic rocks known in the British Isles.

Wales and Shropshire

According to T. G. Williams the *D. bifidus* Beds to the east of the St. David's area contain ashy beds including a volcanic conglomerate as well as massive rhyolitic and keratophyric lithic, crystal and vitric tuffs, many of which are extremely fine-grained.

At or near the base of the *D. bifidus* Beds in the Abereiddy and Abercastle district Cox described the Ynys Castell Ashes which consist of a grit or breccia at the base followed upwards by siliceous or even porcellanous beds showing gradations from lithic tuffs to sediments.

The *D. bifidus* Beds near Carmarthen contain ash beds with broken crystals of oligoclase.

From about this horizon at Llandeilo A. Williams has described beds up to 14 ft. thick of fine-grained vitric to crystal rhyolitic tuffs.

At Fairbourne B. Jones found an agglomeratic ash up to 150 ft. thick in the *D. bifidus* Beds.

Comparable ash beds in the Arthog-Cader Idris district are present in the Bryn Brith Grits and Ashes and the Crogenen Slates. Frequently only bands of a fraction of an inch to a few inches occur but in places they are thicker and include rhyolitic or chinastone ashes. Their source was believed by Cox and Wells to lie to the east of Dolgelley.

In west Shropshire Whittard has mapped bands of water-deposited chinastone ashes in the Hope Shales. A little higher, but still within the *D. bifidus* Zone, the Stapeley Ash attains a thickness of 417 ft. Blyth divided the beds into a number of

types of pyroclastic material ranging from dust tuffs to agglomerates and including lithic, crystal and vitric fragments derived from andesitic, felsitic and aplitic rocks. He suggested that the tuffs represent a considerable number of separate eruptions and recognised a neighbouring vent-agglomerate as a likely source.

A more important development of volcanic rocks occurs a little higher in the sequence around St. David's, the Prescelly Hills, Abereiddy and in the western part of Carmarthenshire, where the Llanrian Volcanic Group and its equivalent the Fishguard Volcanic Series commenced to form before the end of *D. bifidus* times. Farther to the east the approximately corresponding beds are the Fairfach Group of Llandeilo and the Builth Volcanic Series. In North Wales the higher members of the Rhiw Series of Sarn probably lie at about the same level.

The Llanrian volcanic rocks of Abereiddy and Abercastle are between 400 and 500 ft. thick and are mainly pyroclastic, though they include some flows of rhyolite. They are interbedded with dark shales carrying a fauna of *D. bifidus* age. Most of the tuffs are fine-grained but some contain crystal fragments or lapilli and many are mixed with sediment. The lavas are flinty rocks with conchoidal fracture, rarely banded and generally very fine-grained. Cox would classify some of these flows as quartz-keratophyres. Individual flows are thin but of small extent so that the group is very variable, the flows being separated by tuff bands in places.

South of Abereiddy Bay the place of the Llanrian Group is apparently taken by the *murchisoni* Ash, which is an acid rock, 150 ft. thick, containing pumiceous lapilli associated with some felsitic lavas. It, however, appears to come on at the very top of the *D. bifidus* Zone and so represents only the highest beds of the Llanrian Group. The *murchisoni* Ash also occurs north of St. Clears, near Carmarthen, where it is up to 400 ft. thick as mapped by the Geological Survey.

When followed to Ramsay Island the Llanrian Group is found to reach a thickness of from 1,200 to 1,400 ft. according to the calculations of Cox and Pringle.

The Fishguard Volcanic Series is the equivalent of the Llanrian Group and is present in force west of Fishguard on Pen Caer, where Cox recognised three divisions. The lower rhyolites are succeeded by basic ashes, agglomerates and by pillow lavas which, being good spilites, show that the basic magmas so prolific in Arenig times were still furnishing material for the volcanoes. The upper rhyolites follow above the basic beds. G. E. and T. M. Thomas have recently given the thickness of the group as 850–3,600 ft. The spilitic division in places reaches 3,000 ft. and includes pillow and columnar lavas, lava-slag bands, agglomerates and an intra-formational series of feldspar-sands, ashes, slates and flinty mudstones. The rhyolitic members include flinty, flow-banded, nodular and perlitic flows accompanied by pyroclastic beds. East of Fishguard Oliver has recognised welded tuffs.

In the Prescelly Hills to the east W. D. Evans found that the pillow lavas of Pen Caer have died out and the group consists entirely of flow-brecciated rhyolites, vesicular, spherulitic and generally albitised, which are associated with rhyolitic tuffs. The latter vary from banded tuffs to slate agglomerates with soda-orthoclase fragments and also include vitro-lithic tuffs.

At Llandeilo this outburst of vulcanicity appears to be represented by the Fairfach Group, the upper part of which, according to A. Williams, consists of up to 246 ft. of rhyolitic lithic and crystal ashes, conglomerates and albite-rhyolite lavas. The advent of this igneous activity is marked by a break in deposition, but the rhyolitic conglomerates which mark its completion were the products of erosion of the underlying rhyolites. They are followed unconformably by Llandeilo Beds, thus providing evidence of uplift and erosion within the Ordovician of this district, which Williams has likened to a volcanic island-complex.

The presence of earth-movements and erosion within the Ordovician noted at Llandeilo had earlier received striking recognition near Builth Wells. Here O. T.

Jones and Pugh established the succession of upper Llanvirn beds. Resting, possibly unconformably, on shales of the zone of *D. bifidus* they found the rhyolitic ash of the Llandrindod Volcanic Series. This in turn is overlain unconformably by the Builth Volcanic Series which is claimed to be of *D. murchisoni* age and includes keratophyres, breccias, agglomerates and spilites nearly 1,700 ft. thick. Of even greater interest is the unconformable relationship of the overlying Newstead Series, also of *D. murchisoni* age, which consists of boulder beds and sands 520 ft. thick. These beds are banked up against an old sea-cliff eroded in the Builth Volcanic Series, the boulder beds being in fact beach-deposits; they progressively overlap one another and the group as a whole oversteps the various members of the Builth Series. There are remarkable examples of sea-stacks and cliffs as well as wave-worn surfaces and detailed study has enabled the authors to reconstitute phases of elevation, erosion and depression, all of which are confined to the *D. murchisoni* Moment. Still belonging to the same zone, but lying above the Newstead Series, lies the Cwm Amliew Series of rhyolitic ash and ashy mudstone, 150 ft. thick, bearing witness to renewed igneous activity. A further unconformity separates these beds from the overlying black shales with *Glyptograptus teretiusculus* and *Nemagraptus gracilis*.

Outbursts of the same age as those which produced the Builth Volcanic Series probably gave rise to the Cefn Hir Ashes of Fairbourne, Cader Idris and Dolgelley, though the latter were no doubt erupted from different vents. B. Jones described them near Fairbourne as including acid ash and a few beds of agglomerate, estimated to reach 300 ft., but thinning out southwards. They are about 500 ft. thick on Cader Idris, where they include massive beds of fine-grained andesitic dust and agglomerates with rhyolitic or andesitic lapilli. Though apparently missing on Rhobell Fawr they are perhaps represented on Arenig by the lower part of the Lower Ashes and Agglomerates of Fearnsides, 480–580 ft. thick, which comprise well-bedded ashes at the base, passing up into an agglomerate consisting of blocks of hypersthene-andesite. Above this are the upper platy ashes.

Between Fairbourne and Rhobell Fawr a great group of basic igneous rocks is developed above the Cefn Hir Ashes. Near Fairbourne B. Jones named these the Basic Volcanic Group and described them as spilitic lavas and ashes. The lowest thin flows of spilite are not pillow lavas but a later group, separated from the earlier by chloritic ashes, have this structure. A thickness of 1,200 ft. is mentioned but the group thins to the south-west. On Cader Idris it is represented by the Llyn y Gafr Group of spilites, ashes and agglomerates, which attains a thickness of 1,500 ft. but thins rapidly eastwards and westwards by failure of the individual flows. Cox noted the columnar structure of the lower and middle portions of most of the spilite flows and the excellent pillow structure displayed by their tops. It seems likely that at Arenig the equivalent beds are to be sought among the Lower Ashes, though they are chemically different.

In Anglesey no volcanic rocks are recorded by Greenly among sediments of this age.

Lake District

Completely dwarfing any eruptions known to have occurred in the British Isles in Ordovician times are those which broke out at this time and produced the volcanic rocks which now form most of the high fells of the Lake District as well as part of the Cross Fell Inlier. These rocks, of Llanvirn and perhaps Llandeilo age, are known as the Borrowdale Volcanic Series. Pyroclastic rocks, ranging from agglomerates to the finest dust, predominate and include lithic and crystal varieties as well as rarer vitric tuffs and welded tuffs. Only in the very lowest beds are sedimentary fragments found among the lapilli which make up a large proportion of the material. The majority of fragments are andesitic and closely

comparable with some of the accompanying lava-flows. Frequently both coarser and finer tuffs contain a considerable proportion of rhyolitic fragments similar to the acid lava-flows. It is clear that eruptions were proceeding from a number of sources and that the products were andesitic in some and rhyolitic in others, but the main centres of eruption remain unknown. The associated lavas, which with the tuffs attain a maximum thickness of upwards of 12,000 ft., vary from bytownite-basalts to rhyolites, but include augite-andesites and more acid andesites which are the most common lavas. Flow-brecciation is widespread. Secondary alteration with production of albite, epidote, chlorite and associated minerals is common to both lavas and tuffs, while the former show much devitrification. Though the earliest Borrowdale rocks are interbedded with the Skiddaw Slates carrying *D. bifidus* at Cross Fell and so clearly submarine, the vast thickness of this great pile was probably largely subaerial and includes welded tuffs formed as products of nuées ardentes. Some tuffs show current bedding and wavy bedding, however, which suggests their deposition in lakes or sea, but the absence of sedimentary material rules out the possibility of any lengthy submergence beneath the sea during deposition.

It seems likely that the youngest members of the Borrowdale Series have been eroded or lie hidden beneath newer strata, for the succeeding Caradoc or Ashgill beds of the Coniston Limestone Group rest with striking unconformity on earlier rocks. Almost, if not all of the known Borrowdale rocks are transgressed by the Coniston Limestone between Shap and Dalton in Furness. The age of the gap represented by the unconformity cannot yet be fixed with certainty, but the amount of pre-Caradoc erosion demands an appreciable period, presumably at least in part Llandeilo.

Llandeilo

It is doubtful if Llandeilo rocks are present in Ireland though the Series is well developed in South Wales, where, however, volcanic eruptions would seem to have died down. The only certain evidence of the existence of rocks of this age in North Wales seems to be in the Berwyn Hills. As already mentioned it is likely that considerable earth-movements and subsequent erosion affecting the rocks of the Lake District are of this date and some authors have claimed the same for southern Scotland. Such an unconformity may well also exist in Wales.

Wales

The *Asaphus* Ash, at the base of the Llandeilo, attains a thickness of about 70 ft. in the neighbourhood of Carmarthen, where it consists of andesitic tuffs, agglomerates and ashy sediments. Westwards and eastwards it disappears.

There is no sign of volcanic activity in the Llandeilo area.

In the Berwyn Hills the Llandeilo sequence of some 2,000 ft. or more includes several thin ash beds and a lava-flow. The Llandeilo Ash at the base is from 200 to 230 ft. thick. Higher in the succession come the Garwallt Ash up to 20 ft. thick; the 25- to 80-ft. ash beds of the Gwern-feifod Group, which include pumiceous ash and bombs, as well as feldspar crystals and an associated vesicular flow-brecciated lava 60 ft. thick; more porcellanous ashes and finally the Mynydd-tarw Ash.

The Llandeilo Shales of Anglesey are devoid of volcanic material.

Caradoc

Though more detailed work is required in Ireland many of the volcanic rocks of Tyrone, the Dublin district and the tract extending from Wicklow to Tramore are probably of Caradoc age. The chief centres of eruption were, however, in Snowdonia. There were outbursts marked by deposits near Dolgelley, in the Breidden

and Berwyn Hills and in Shropshire, but there is no record from South Wales or Anglesey. Only minor outbreaks occurred in the Lake District, but volcanoes were active in the Southern Uplands of Scotland.

Ireland

Caradoc volcanic rocks in Ireland bear witness to eruptions in Tyrone, near Dublin and in Waterford and Wexford. The precise age of some of these strata is still uncertain and the record in places of fossils of earlier date is a warning that too much reliance must not be placed on the classification here adopted.

The Tyrone rocks were shown by Hartley to include flow-brecciated hornblende-andesites and trachytes, both of which are, however, subordinate to basalts and spilites showing pillow structure. All the lavas appear to have been thin flows probably originating to the east of their present outcrops and together amounting to 400 or 500 ft. in thickness, which figure includes some lithic and crystal tuffs. Agglomerates consisting of black cherty shale or of felsite and basic lava rest unconformably on the pillow lavas and tuffs and afford evidence of later, but still, perhaps, Ordovician activity. There is some doubt about the exact age of the group which may be upper Llandeilo or lower Caradoc, but Ashgill Beds rest unconformably on Tyrone Beds at Pomeroy some twelve miles away.

At Collon, in County Louth and Grangegeeth in County Meath, the Grangegeeth Volcanic Series has been described by Harper and Manistre. The volcanic rocks underlie black shales with *Dicranograptus clingani* and *Pleurograptus linearis* and are thus probably roughly of the age of the Snowdon volcanic rocks. They consist of basalts and auto-brecciated keratophyres with coarse and fine crystal tuffs.

Similar rocks appear at Kildare where Reynolds and Gardiner described augite- and hypersthene-andesites associated with ashes and thought to be submarine.

At Balbriggan, County Dublin, Gardiner recorded much decomposed augite-andesites interbedded with fine and coarse ashes containing fragments of amygdaloidal lava, the group being perhaps as much as 5,000 ft. thick.

Farther south at Portraine, Gardiner and Reynolds found andesite flows with much altered crystals of labradorite and augite. The sequence also includes coarse ashy conglomerates and calcareous ash, interbedded with ashy limestone and shale.

At Lambay Island the same authors thought the volcanic rocks were submarine. They include tuffs of varying degrees of coarseness, some of which are pumiceous and others perhaps mark the positions of vents. Also present are vesicular augite-andesite flows, sometimes containing igneous inclusions, and breccias. There is an unusual rock known as Lambay Porphyry with large platy feldspars, allied to labradorite, and augite. A 'Bala' age for the group is suggested but not proved.

There is a belt of Bala volcanic rocks running parallel to the strike for many miles from near Wicklow south-westwards and a second line starting from Arklow Head and extending nearly to Dungarvan in Waterford 80 miles to the south-west. In the Waterford district Cowper Read noted that the Ordovician rocks between Tramore and Ballyvoyle Head include nodular felsites, which are presumably flows, and tuffs. Fossils in adjoining beds suggest that much of the group is 'Bala' but an earlier age is not ruled out.

Wales and Shropshire

Though the main activity at this time was undoubtedly in Snowdonia there appears to have been at least one other centre near Dolgelley, completely separate from the former. Thus on Cader Idris and in the Rhobell Fawr area the Upper Basic Group consists of vesicular, spilitic pillow lavas associated with tuff bands, which in the central and eastern parts of the district are subsidiary, but become predominant in the west. The group is from 400 to 500 ft. thick. Wells' Main Volcanic Group of Rhobell Fawr is evidently the attenuated representative

of this outbreak, with coarse explosion tuffs having a matrix of crystal tuff, which probably originated in a volcano to the east of the mountain. Spilitic breccias and spilite lavas also occur, the latter with pillow structure. These volcanic rocks did not apparently extend into the Arenig country though the same is probably not true of the higher, but still apparently Caradoc, volcanic rocks of Dolgelley, known as the Upper Acid Group. The latter are not found in the Fairbourne district where B. Jones thought their absence might be due to faulting, though it is possible that they were never deposited. On Cader Idris they attain a thickness of 1,500 ft., made up of rhyolitic and acid andesitic ashes and lavas with interbedded slates. The rocks are less acid than the Lower Acid Group and include keratophyres, usually much silicified, as well as rhyolitic dust tuffs. These beds are the same as the Upper Acid Volcanic Group of Rhobell Fawr, mainly pyroclastic, but also including nodular, flow-banded and brecciated rhyolites.

The Upper Ashes of Arenig, about 1,300 ft. thick, are very likely on this horizon. Fearnsides showed that they consist in the lowest part of agglomerates with thin andesitic lava-flows, above which are more ashes, finer in grain with many streaks—characters suggesting welded tuffs. The highest beds are more siliceous ashes with chips of feldspar crystals.

Activity in Merioneth evidently waned fairly early in Caradoc times for the last outbursts are represented by the Perfeddnant Ashes, two beds about 40 ft. thick, mapped by Jehu near Towyn and the Frondderw, Pont-y-Ceunant and Gelli-Grin ashes of Bala, which Elles recorded as respectively 12, 25 and up to 40 ft. thick. These beds, with lava fragments, have restricted outcrops.

Farther south, at Llanwrtyd Wells the Lower Ashes of Stamp and Wooldridge appear to be of this age. They consist of ashes below, followed upwards by a rhyolitic breccia, probably an agglomerate, but perhaps a flow-breccia, the full sequence totalling about 125 ft. Separated from these beds by shales with *Dicranograptus rectus* are the Spilites and the Upper Ashes, the pyroclastic beds of the former containing blocks of spilite resembling the higher flows, which show pillow structure. They are 100 ft. thick. Mudstones intervene between them and the Upper Ashes which are about 40 ft. thick, acid in composition with quartz and feldspar crystal tuffs, some halleflinta and quartz-keratophyre tuffs.

To the north in Snowdonia the first mutterings of the great storm of volcanic activity of Caradoc times resulted in the eruption of the Talgau lavas of Llanberis. According to D. and H. Williams these beds are about 600 ft. thick at their maximum and include impersistent flows of keratophyre and soda-rhyolite, parted in places by grit. They are represented by 1,500 ft. of beds near Capel Curig and on Glyder Fach. They are more sodic than the mass of the Snowdonian lavas above them and a tendency to albitisation is marked. Near Tremadoc andesitic lavas and ashes among the early Caradoc beds were described by Fearnsides, who also mapped rhyolitic ashes a little higher in the sequence near Criccieth.

At Trefriw D. A. B. Davies found that the Glanrafon Beds include intercalated thin rhyolite flows and tuff bands, as well as the spilitic agglomerate locally developed at Capel Curig. The flows are aphanitic, flinty and usually less than 20 ft. thick, while the pyroclastic rocks are vitric tuffs and crystal-vitric tuffs with rhyolite lapilli.

H. Williams and Bulman demonstrated that the comparable beds at Dolwyllelan include lenses of rhyolite and rhyolitic tuff.

Around Conway Elles described the Lower Banded Lava Group which is approximately of this age. It consists of some 1,400 ft. of rhyolitic rocks, flinty, fine-grained and banded, which may be flows but about which some doubt exists, save in the case of some thin flows with phenocrysts of orthoclase and oligoclase in a trachytic base. These beds, however, are a little earlier than the main mass and were erupted from a separate vent.

Harker long ago pointed out that the Bala Volcanic Series was derived from a number of vents along the margin of the ancient pre-Cambrian ridge of Caernarvonshire. Probably volcanic activity producing similar types of lavas and tuffs started at slightly different times at the several centres. It may yet turn out that some of the volcanic rocks are in fact of Llandeilo age as has been claimed for example by D. A. B. Davies near Trefriw. Whatever their exact age the Lower Lava of Conway is followed by the Lower Ash, above which lie the Upper Lavas of Conway, 710 ft. thick and consisting of flow-brecciated and nodular rhyolites. The highest beds of the Conway Mountain Volcanic Group, as worked out by Elles, are the ashy grits and interbedded rhyolite flows of the Upper Ash Group.

The Crafnant Volcanic Series of Trefriw, described by D. A. B. Davies, includes rhyolites and rhyolitic vitric, lithic and crystal tuffs, and even agglomerates, interbedded with sedimentary slates, the whole attaining a thickness of 1,650 ft. Included within it is a spilitic agglomerate and bands of chloritic pumice tuff.

At least the upper part of the Conway-Crafnant Volcanic rocks is likely to be the equivalent of the lowest of the Snowdonian Volcanic Suite. H. Williams and Bulman have demonstrated the approximate southward extent of several of the Snowdonian lava-flows, which were evidently submarine. Thus the lowest of the Snowdon lavas did not reach as far as the Dolwyllelan Syncline where only the uppermost Snowdonian rocks are present.

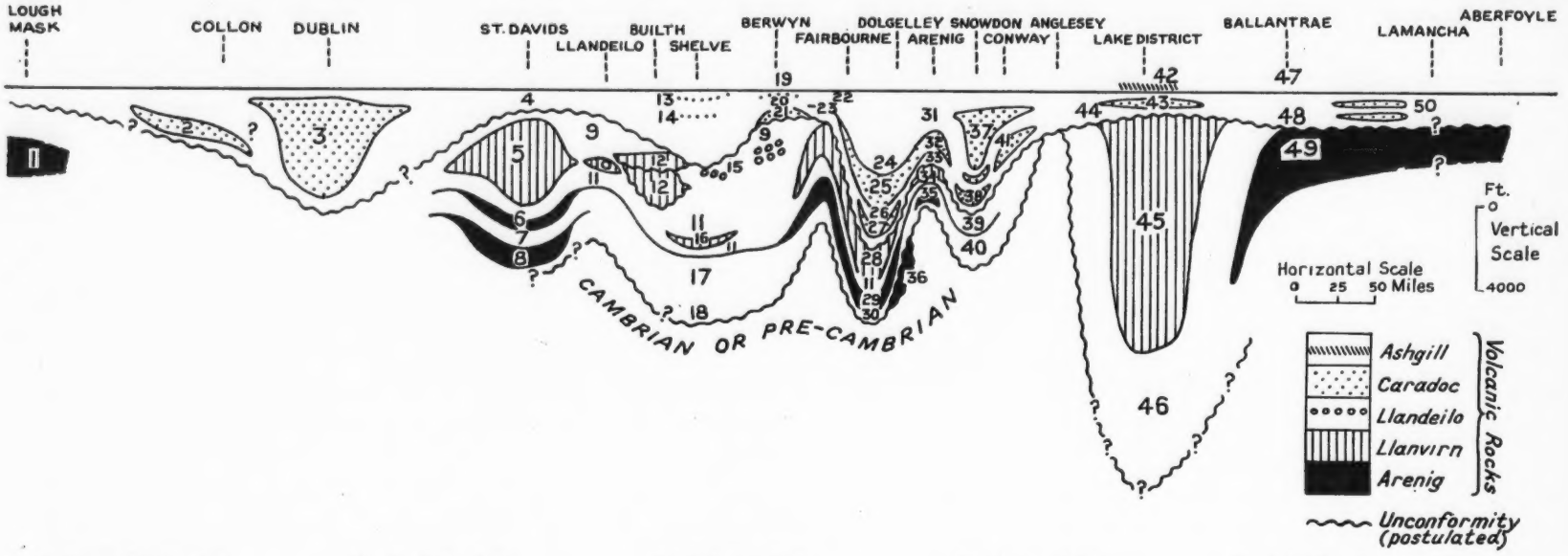
The Lower Rhyolite Series of Snowdonia, according to H. Williams, includes nodular rhyolites some 20 ft. thick at the base, followed by the Pitt's Head lavas, which are banded potash rhyolites up to 200 ft. thick. The succeeding massive flinty rhyolites are associated with rhyolite tuffs or coarse lapilli tuffs and reach 400 ft. in thickness. Above the rhyolites come up to 1,500 ft. of unstratified Peléan type tuffs which thin out rapidly towards Beddgelert and pass into slates. The Bedded Pyroclastic Series which succeeds may reach as much as 1,200 ft. in thickness and is composed of pumice tuffs of andesitic or rhyolitic composition with lapilli and bombs of keratophyre and spilite. These tuffs are interbedded with rhyolite tuffs, thin rhyolite flows and sediments. The lavas are ropy and show slaggy flow-banding and the group is evidently submarine. An important point is the evidence for simultaneous extrusion of potash-rich acid, andesitic and basic, as well as spilitic material. In eastern Snowdonia D. Williams noted an andesitic basalt flow nearly 200 ft. thick. The highest of the Snowdonian lavas, the Upper Rhyolitic Series, is made up of 300 ft. of potash rhyolites and tuffs. The lower beds are tuffs with rhyolite and keratophyre fragments and also halleflinta-like vitric tuffs. The lavas are flow-banded rhyolites which are sometimes vesicular.

In the Breidden Hills the Lower and Upper Volcanic Groups, both consisting of ashes and chinastones, are placed by Whittard low in the Caradoc, being separated by shales. The upper contains in addition an andesitic conglomerate and at about this level is the thin trachybasalt noted by Whittard as associated with Harnage Shales near Craven Arms. Rather higher are the Hagley and Whittery volcanic groups of acid and andesitic, water-deposited tuffs of the Shelve country, respectively 350 and 300 ft. thick.

In the Berwyn Hills, according to the Geological Survey, the Caradoc Series contains the Cwmclwyd Ash of fine and coarse rhyolitic or keratophyric tuff, up to 700 ft. thick at the base. Higher in the succession come the Pandy Ash with 60 ft. of keratophyric tuff and the Pen-y-graig Ash of the same composition, but only 20 ft. thick.

Lake District

Volcanic activity in Caradoc times in the Lake District was confined to a 450-ft. flow of acid, flinty rhyolite, the Stockdale Rhyolite, only developed in the east of the district.



1. Mount Partry Beds
2. Grangegeeth Volcanic Series
3. Balbriggan, Portraine, Lambay
4. Castell Limestone
5. Fishguard Volcanic Series
6. Sealyham Group
7. *Tetragraptus* Beds
8. Trefgarn Andesites
9. Llandeilo Beds
10. Fairfach Group
11. *D. bifidus* Beds
12. Builth Volcanic Series
13. Whittery Ash
14. Hagley Ash
15. Meadowtown Ash
16. Stapeley Ash
17. Mytton Flags
18. Stiperstones Quartzite
19. Dolhir Beds
20. Bryn Beds
21. Cwm Clwyd Ash
22. Ceiswyn Beds
23. Llyn y Gadr Mudstones
24. Tal-y-llyn Mudstones
25. Upper Acid Group
26. Llyn Cau Mudstones
27. Upper Basic Group
28. Lower Basic Group and Cefn Hir Ashes
29. Lower Acid Group
30. Basement Group
31. Nant Hir Shales
32. Upper Ashes
33. Daerfawr Shales
34. Lower Ashes
35. *Calymene* Ash
36. Rhobell Fawr Group
37. Snowdonian Volcanic Suite
38. Talgau Lavas
39. Maesgwm Beds
40. Plas-y-Nant Beds
41. Lower Conway Lavas
42. Ashgill Ash
43. Stockdale Rhyolite
44. Stile End Beds
45. Borrowdale Volcanic Series
46. Skiddaw Slates
47. UpperWhitehouse Group
48. Kirkland Group
49. Ballantrae Lavas and Tuffs
50. Tweeddale Lavas

DIAGRAMMATICAL SECTION SHOWING VARIATION IN ORDOVICIAN VOLCANIC ROCKS

Scotland

In southern Scotland the great unconformity already noted in the Lake District is found, according to Pringle, who claimed that Caradoc and Ashgill strata rest with discordance on folded Arenig beds. Volcanic rocks appear within the Caradoc beds at two horizons. The lower is in the Glenkiln Shales at Bail Hill, Sanquhar, where both pyroclastic rocks and hornblende-andesites occur. Another, much brecciated rock carries large crystals of augite and is perhaps a flow-breccia. A vent is suggested at one place. Similar tuffs with andesite fragments occur near Ballantrae, at Broadsea Bay in Galloway and also near Sanquhar and Heriot. The higher horizon is in the upper Caradoc of the Tweed valley where the Tweeddale Lavas are up to 100 ft. thick and include flows with trachytic structure and ashes with similar fragments.

ASHGILL

Evidence of the dying outbursts of Ordovician vulcanism is to be found in the Ashgill Series of the Lake District and near Wrexham.

Near Coniston the Ashgill includes a thin bed of yellow rhyolitic ash, 10 ft. thick, probably water-laid. Similar ashy beds are found at Cautley near Sedbergh, where they accompany thin flows of rhyolite, the whole being some 60 to 70 ft. thick. Contemporaneous volcanic ashes were mapped by King and Wilcockson in the Ashgill of Wharfe near Austwick, Yorkshire. A thin tuff band also occurs in the Cyrn-y-brain Beds near Wrexham.

Enough has been said to show that the stratigraphy of the Ordovician System is complicated and that thicknesses, particularly of the volcanic rocks, vary greatly from place to place. It seems at least likely that a good deal of the difficulty encountered may be due to the presence of considerable unconformities within the system.

I have tried briefly to give some account of a short chapter in the history of that part of the earth's crust which now forms the British Isles. At times during that period existence must have been distinctly uncomfortable for the creatures which inhabited the waters around the volcanoes, but life still went on. In these days of atom-splitting and artificial satellites it is salutary to ponder awhile on the gigantic forces of nature and to realise how little we yet know about their causes and how far we are from being able to control them.

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THE EVOLUTION OF ORGANISATION WITHIN THE NERVOUS SYSTEM[1]

ADDRESS BY PROFESSOR J. Z. YOUNG, F.R.S.

PRESIDENT OF SECTION D

1. The Control of Living Processes

Before we can discuss the evolution of organisation within the nervous system we shall have to deal with the wider problem of finding methods suitable for the description of any living organisation. The attempt to study the nervous system as if it were distinct and somehow different from all other parts of the body weakens both neurology and general biology, whereas each instead can greatly help the other. The nervous system is an agency that controls the actions of the body. It does so by communicating information in a code of signals, the nerve impulses. Moreover, many nervous systems can store information; that is to say, can keep records or representations of past events and use them to take actions that accurately forecast the future.

We are not in the habit of considering that other parts of the body have these properties of control, signalling, memory and prediction, but I propose to show that they are quite general. All life involves a property that we may call control. All living systems can be usefully considered as communication channels, transmitting information in codes, which comprise systems of signals. Many parts of the body besides the nervous system can properly and usefully be considered to possess memories.

These claims may seem to be so wide that they are absurd and you may suspect that they are dictated only by a preference for a new fashion of introducing communication theory here, there and everywhere. It is indeed necessary to restrain enthusiasm in the use of so powerful a language. But I hope that sober consideration will show that such an approach provides Biology with a tool *for which at present no other effective substitute has been suggested.* Perhaps equally important is the hope that understanding of the pervasive nature of the communication process in all life will show those who deal with the mathematical and applied aspects of information theory the way to more fundamental definitions and treatments of the subject.

When thinking about the characteristics of living organisms we cannot indefinitely evade consideration of their predictive powers. We can of course make many useful descriptions of parts of organisms with conventional physical and chemical methods. But some biologists at least must pay attention to the fact that organisms are exceedingly complicated systems, which perform elaborate and varied actions. The biologist needs to describe these actions in their totality. Seeing a swallow swooping accurately backwards and forwards, aiming at the insects as it passes, he needs a method of description beyond those that he would find in study of the physics of flight or the chemistry of the bird's molecules. Nowhere in any textbooks of biochemistry or biophysics will he find terms suitable for the description of a whole living bird. The biologist cannot evade the fact that the actions of organisms are *adaptive*—that is, they tend (in general) to promote the survival of the organism or of the race. Or, to put it in another way, the organism takes actions that will ensure its survival if the future resembles the past. To say this is to imply that there is sense in saying that an organism 'could do' several things and that it

[1] Address delivered at the Dublin Meeting of the British Association on Thursday, September 5, 1957.

'chooses' the appropriate ones. I suggest that this is a suitable and indeed necessary way for talking about complicated self-adapting systems such as organisms. It is *not* a way of speaking that is adopted by classical physics or chemistry, and therefore their biological offshoots have not yet been able to handle such matters. Some progress in the discovery of exact methods for the study of such questions of choice has however been made by those who study language and its communication and we may attempt to borrow their methods.

The problem of describing the predictive or adaptive character of living things is especially acute for embryologists and students of evolution, who must find language with which to describe the changes that the organism undergoes in development and in phylogeny. As the child develops within the womb, lungs are formed before there is any air to breathe, stomach before there is food to eat, legs before there is anything to walk on, eyes while all is still dark, and a brain with nothing much yet to think about. So through countless examples we find everywhere in living systems what seems to be 'forecasting'; everyhwere there is what we rather feebly call 'adaptation'.

When I say that current physics and chemistry do not help us to solve these problems I must not for one moment be taken to mean that these sciences have nothing to give to biology. On the contrary, great advances are coming largely because biology is learning to apply the exact methods by which physical science isolates specific aspects of situations and describes them in strictly denotational terms—terms with a rigid and exact meaning that should allow the same predictions by every rational person who understands the language. The essence of this method lies in the isolation of phenomena, allowing accurate study. It is a humble method and it does not aim at completeness. For this very reason it cannot be, by itself, in its present form an adequate method for biology. Living organisms are characterised by complexity. They are aggregates of various materials, which, though continually interchanging with their surroundings, yet remain in a certain sense the same, or at least change little. However much detail we acquire about the chemical composition and physical properties of parts of the system over limited periods of its existence, there still remains for science the problem of describing the extraordinary control process by which the whole organism or race of organisms maintains its constancy.

2. Control with Different Time Scales

We can now begin to see how this control of the stability of life is exerted at various time-scales. Metabolism is controlled from second to second within the cells. The actions of the various parts of the body are controlled by the nervous system or, more slowly, by the chemical signalling of the endocrine system. By a still slower process, individual tissues regulate themselves to suit the demands likely to be made on them, muscles grow large with use, blood contains more haemoglobin at high altitudes, and so on.

Any system of control implies a controller, some agency that regulates performance in such a way as to ensure a particular end or ends. In the case of living things this end seems to be the maintenance of the stability of the race, and the controller is basically the nucleoprotein materials that constitute the genes, the inherited set of instructions of the organism. If instructions are to be effective they must deal with relevant matters, and so we may say that these nuclear materials constitute a coded representation of those aspects of the world that are relevant for the life of the species. They are kept up to date only if they are periodically reshuffled and changed by the sexual process and by mutation. New organisms must then be built by the processes of embryology, providing adults ready to try out slightly different variants of the control process. So, on a very long time-scale evolution provides organisms that are able to survive in a changing world.

If we are right in calling attention to the fact that all these processes share the attribute that they are controlled, then we should be able to define this control more precisely and be led to make appropriate studies of the variables of the process in each of these fields. I believe that we can in fact do this and give not only a valuable common language for talking about all these things but ideas that will show us what it is useful to study about each of them.

The conception that a process is controlled comes of course in the first instance from the control that human beings exercise upon each other and over the artefacts that they make. The rapid recent elaborations of these artefacts has made it necessary for mathematicians and engineers to refine and elaborate the concept of control, and we in biology are today experimenting with the use of their techniques in our subject. They in turn may perhaps gain further ideas from looking backwards at the living control systems from which they and their artefacts have all sprung.

The concept that a system is controlled implies that it is at least sufficiently complicated to be able to operate in several different ways. For us to be able to say anything very useful about the control we must know something about the various things that the system can do. This obviously implies that the system remains essentially the same at least for a considerable stretch of time, so that we can adequately study its performance. In fact information theory deals with such systems as languages, organisms and complicated machines, which have exactly this persistence and variety of behaviour. It is a form of statistics designed to help in the description of such complicated things as living organisms, human beings and the languages that they produce.

3. Representations and Signals in the Organism

The organism maintains its stability not in spite of the multiplicity of its parts but because of it. Every cell, every individual, every species, every genus, every family, order and phylum has a range of what we might call possible actions, among which some are selected by each situation. This conception of selection among possibles is fundamental alike to information theory and to biology. The process of selection leaves the organism with that suitability for its environment that the biologist terms its adaptation. In other terms, we may say that the organism thus comes by selection to contain a *representation* of the environment. If we could specify the nature of this representation and measure its parameters we should make a great advance in the study of the biological puzzles of adaptation. In order to do this we may use with great effect the mathematical methods of the engineer. We may say that the organism is a communication system handling signals in code. We are quite used to this idea for the nervous system, why not apply it elsewhere in the body? The nervous system is not the only part that is controlled and exerts control; indeed, organisms with no nervous systems show adaptive behaviour and stability.

Study of the principles of coding and transmission of signals shows how close is the similarity between what we do with words, what the nervous system does with nerve impulses, and what all cells do with their parts to maintain stability. A signal may be defined as an aggregate of physical events that has acquired a particular selective value in relation to some set or ensemble of possible alternative actions, such as we have been considering.

4. Control and Signalling within Cells

Thus throughout the organism many of the units act as transmitters of signals. The various specific molecules of the chromosomes provide signals that select among the processes that go on during development. If a system of signals is to be transmitted and replicated without distortion it must consist of discrete units, separated by steps that are large in relation to the margin of error allowed in copying (Mandelbrot,

1955). This breaking up of events within the organism into discrete units is very common. The electron microscope has shown in recent years how much of the living organisation depends upon systems of particles. I have suggested that we may understand these as elements of the signalling system of the cell, either carrying instructions outwards from the nucleus towards the cell surface or information inwards, ultimately to the nucleus (Young, 1956).

When any molecule arrives at the cell surface something has to decide which of several possible courses of metabolism it shall pursue. In ordinary terms we say that this is determined by the enzymes that it happens to meet. Such a description is chemically correct but biologically inadequate. In just the same way it is physically correct to say that the sound 'Stand up' consists of certain defined vibrations of the air. Yet such a physical description of the sound would by itself be hopelessly inadequate for linguistics or psychology, which are the sciences able to forecast what is likely to happen to a human being when this wave-form strikes on his ear. For that purpose it is necessary for these scientists to know the past history of the relation of such sounds to human beings and hence the selective value of the sounds —that is, the way they influence the choice among the numerous things that a person may do. Similarly I suggest that some at least of the particles in cells can be appropriately considered as signals.

The organism remains alive because the enzymes that each incoming molecule meets are not just a random lot collected by chance. They have been selected in the past history of the cell according to a system of what we may call its instructions, indicating which actions are likely to prove adequate if the future resembles the past. I suggest that the continued stability of the organism may be said to depend on the fact that the distribution of these aggregates of molecules within the cell has the properties of a signalling system. If so, there must be many different specific sorts of particle sent out in a manner that is controlled by past events and producing specific selective effects at the sites where they arrive. From a practical point of view this would mean that we must study the history, statistics, transmission and actions of these particles, matters which indeed we should all like to know about in any case. If the cell is truly a controlled system of this sort we should be able to identify its coded instructions, see how it encodes, transmits and stores information and recognises the comparators, output channels and decoders by which it sends out instructions that maintain its stability. Moreover, we should be able to record the extent to which it achieves stability and where it meets limitations due to inertia or overactivity of the controls oscillations, effects of noise and so on.

The significance of many of the particles that are present in a cell may thus be found in the knowledge of the places that they have in a system of signals—as phonemes or words within a language. Such significance can of course only be seen by study of the structure of the system as a whole, and this is perhaps the chief lesson that is to be learned from this analogue. Nothing can be deduced from a single vowel-sound or mark on a piece of paper about their significance as part of a communication system. This can only be found by studying the use of the whole language system over a long period in the past. It seems likely that this is what we shall need to do with the study of the aggregates of molecules that make up cells. In other words, we need a language that will describe the whole of this complicated system—the organism. If anyone does not like the suggestion made here they should supply a better, but to say only that physics and chemistry, as they are at present, can describe organisms is as silly as to say that by themselves they can, with present techniques, describe language.

5. Signalling between Cells

If it is true that living organisation is maintained by the transport of materials that serve as intracellular signals regulating metabolism, then it is not difficult to see

how signals came to be sent from one cell to another and so initiated the evolution of the nervous system. The earliest steps of this development are still unknown to us and it is not profitable to spend long in guessing about them. Even today nerve fibres evoke the activity of their end-organs by releasing specific signalling substances, such as acetylcholine. There are other signs that there is no large gap between chemical signalling by hormones that diffuse through the bloodstream, those that pass through the tissues, and the signalling substances that are released by nervous action (Young, 1934; Huxley, 1955). We can imagine how cells on the outside of an early metazoan organism may have begun to discharge substances that evoked specific activities on the part of specific cells within and thus became the first receptors.

It is hardly possible to guess at the steps by which the first nerve fibres with propagated action potentials arose. The ionic interchanges at the surface, upon which such propagation depends, are presumably specialised versions of the mechanism by which all cells maintain their specific composition in media from which they differ. However, the question of the possible existence of propagated disturbances at the surfaces of cells of such organisms as sponges remains to be investigated. We know only, from the work of Parker and others, that some form of propagation over short distances can occur. By the stage of coelenterates we find already fully developed nerve fibres and some at least of these propagate action potentials (Horridge, 1955). However, they conduct relatively slowly (·2 — 200 cm/sec.) and at low frequencies (5/sec. to 1 in 100 sec.). Moreover, they are not aggregated to constitute any compact central nervous system. They serve to select suitable responses from the rather limited repertoire of the organism. A sea anemone can be extended or be contracted. It may bend over towards food, seize it, engulf it. It makes spontaneous movements of various sorts but takes few or no actions directed towards placing itself in a situation where food or favourable condition will be obtained. The forecasting system nevertheless finds conditions suitable for life, assisted by the mechanisms for distribution of the larvae—which of course themselves involve elaborate and specific selection of responses, though of these we know little.

It is important not to oversimplify the behaviour of even the simpler organisms, and no doubt closer study shows that they are more complicated than at first appears. Nevertheless, it is probably correct to say that their repertoire of responses is limited compared to that we shall find, say, in a vertebrate, an insect or a cephalopod. Moreover, the responses of coelenterates are probably mostly dependent on the hereditary make up or instructions and are influenced little if at all by memories stored within the nervous system.

Pantin showed that the daily rhythm of expansion and contraction continues in the dark, and this implies some memory system. No doubt changes in response pattern with time may occur, just as there is probably adaptation of the signalling system within even protozoan or bacterial cells. We must shortly consider how such adaptive changes are to be considered in relation to learning as we know it in higher animals.

Before we can do this it is useful to contrast higher nervous organisation with that of these simpler forms in order to bring out the nature of the differences. I propose to do this by referring to some features of the nervous system of cephalopods, which Mr. Boycott and I have been investigating recently. It would be ridiculous to try to list all the ways in which such a nervous system differs from that of a sea anemone, but I want to try to bring out some of them. The organisation of a complex nervous system ensures a stable life by the possession of receptors of high resolution that select from a *large* variety of possible responses and by using memories of its own past experience for the selection. How does it do this?

First of all the number of nerve cells involved is very large. In an octopus there are at least 10^8 and in man 10^{10}. This question of number is important

in itself, as providing some measure of the variety of the system within which selection can be made. It is presumably no accident that this increase of number of neurons has occurred independently in several lines during evolutionary history. There must be some great selective advantage, at least for life in certain types of habitat, in this multiplicity of communication channels. It is a pity that we still have so few facts about numbers of nerve cells and their sorts, or about the details of their branching and interconnection in different animals, or in different parts of the nervous system of any one animal. In spite of the great amount of work done, it is not too much to say that the exact quantitative study of the nervous system has only just begun.

In *Octopus*, as in other higher nervous systems, the cells are by no means all alike and we can recognise a hierarchy of types. The motorneurons that directly control the muscles are few and large, occurring mainly in the suboesophageal ganglia, and ganglia of the arms. There are between one and two million neurons in these suboesophageal centres, but many of them are small. If the actual motorneurons that control the muscles be taken as those with nuclei of more than 10μ diameter, then there are perhaps 200,000 of them. Each part of the action system has its own set of motorneurons, and we have been able to identify those for the arms, funnel, mantle, viscera, chromatophores and skin muscles, eye muscles, muscles of the blood-vessels and so on. These are the final motor systems out of which behaviour is compounded. The problem is to find out how selection is made from second to second of the right muscle groups, so as to produce a behaviour system that keeps the animal alive.

In *Octopus* we can identify these higher motor centres with some precision. For example, when the anterior basal lobes of an anaesthetised animal are stimulated electrically, the octopus gets up from the table and makes well-integrated walking movements towards that side. If the tract running to the magnocellular lobe is stimulated the animal adopts the dymantic or terrifying attitude, serving by its sudden appearance to elicit a flight reaction in an attacker. Similarly, the lateral basal lobes are the higher centres for the control of colour patterns, the medial basal lobes for swimming.

The neurons of these higher motor centres are smaller than the motorneurons. There are altogether about a million of them, compared with more than 100 million in the higher centres and some 200,000 motorneurons. The networks of fibres made by their processes show different patterns. At present we have no means of analysing or describing the networks. Indeed, we do not even know how to begin with the task. Somehow they must serve to set off and control the elaborate patterns of action that are involved in swimming or walking towards prey or away from danger, attacking enemies or finding a mate, and so on. It is startling to reflect how little we know about the anatomy or physiology of such higher motor centres. They exist throughout the brain-stem of a vertebrate, where Hess and others have shown that stimulation in appropriate areas can release elaborate patterns of response. Yet we have little knowledge of how the connections evoke the patterns or how it is ensured that actions are performed in a steady and well-regulated manner. The case of the postural mechanisms of the neck and limbs of vertebrates is one of the best-known examples, but even here the data on the connection patterns within the centres are very scanty and there is little information about the firing frequencies of the units concerned. We have been concerned in the past only to look at the grosser features of nervous organisation and there is an immense amount to be done to probe the details, without which it is not really possible to say that we begin to understand the system of control.

So far we have said nothing about the mechanism that selects which actions shall be performed by an octopus. The main receptors are the eyes and touch and chemical receptors of the arms, assisted of course by statocysts and other receptors.

There is no information available about the capacity of these systems at hatching in *Octopus*, though Wells has investigated the question in *Sepia*. When octopuses are investigated shortly after they have been brought to the laboratory from the sea it is found that they sometimes attack any small moving object. The frequency of attacks differs for different octopuses and for different moving shapes. In one series of twenty-two octopuses the attacks made at a selection of figures by individuals varied from 14 per cent to 90 per cent. No rewards were given for these attacks. They were made most often at moving live crabs (85 per cent), and then with decreasing frequency at white circles (67 per cent), small white rectangles, 10 cm. by 2 cm. (64 per cent), black circles (57 per cent), black rectangles (49 per cent), large white rectangles, 20 cm. by 4 cm. (38 per cent).

The optic system of these animals is thus in a state that ensures with a probability less than 1 that crabs and similar objects will be attacked in a new situation. However, this probability is influenced by many other factors, and in particular and in a most curious way by the food that is taken. If a set of octopuses that has been attacking a given figure on a small proportion of occasions (say 20 per cent) is given food, then for the period shortly thereafter the probability of attack on all figures is greatly increased (to 80 per cent or more). This effect quickly passes off however. But if a figure is shown on several occasions shortly *before* giving food, then the probability of attack on that figure is specifically increased for a long period. We then say that the octopus has learnt to attack this figure. Other figures continue to be attacked only seldom. Some neural change must therefore have taken place, presumably a facilitation along certain pathways, increasing the probability that there will be an attack when a certain pattern of signals arrives from the retina. The result of this set of changes in the nervous system we refer to as a memory producing attack on the figure giving this pattern of signals.

If an electric shock is given to an octopus, then during the ensuing period of hours the probability of attack on moving figures is lowered. If a given figure is shown several times during this period and further shocks are given when it is attacked, then this figure will be seldom or never attacked in the subsequent days. A memory *preventing* attack on it has thus been set up.

Octopuses are able in this way to acquire memories that ensure that they shall attack one but not the other of at least three pairs of figures, namely large and small squares, horizontal and vertical rectangles and black and white circles. This is certainly not the limit of their capacity, though it is interesting that some of these memories interfere with each other, especially while they are in process of formation.

The setting up of the memory involves the facilitation of certain pathways that are used during the period for which the excitability of this part of the system is raised by recent feeding, or conversely for memories that prevent attack. The information that ensures that correct behaviour shall be selected is stored in the form of a changed excitability in cells scattered through the higher centres of the nervous system.

We still do not know exactly what is involved in these changes in facilitation within the nervous system, but it has proved possible by operations to influence the process of learning in revealing ways. The optic lobes of octopus are a mass of tissue having each some sixty million neurons of various sizes, many of them very small. Their axons pass through a plexiform arrangement to the superior frontal lobe, with two million cells, and from this other axons through further plexuses to the vertical lobes. The latter have about twenty-five million cells, many again very small. Their larger fibres, however, pass impulses back through a further synapse in a subvertical lobe (one million cells) to the optic lobes.

We have known for some years that the vertical lobes are concerned with the setting up of memories, but it is now possible to be much more specific and to say something of how they operate to alter the probabilities of attack at objects that

have particular visual properties (Boycott and Young) or tactile properties (Wells and Wells). Octopuses from which these lobes have been removed still come out more often during the period after they have been fed and less often after a shock, but the effects last for a much shorter time than they do in a normal animal. Octopuses without these lobes learn only very slowly or not at all if they are given trials at long intervals (more than one hour). However, if the trials are given within a few minutes of each other, then the animals may learn either visual or tactile discriminations to a moderate level of accuracy, though always below normal. The vertical lobes therefore clearly have something to do with maintaining within the nervous system a representation of events of the recent past. But if the same events are repeated at short intervals, memories can be set up elsewhere in the nervous system. However, the vertical lobes cannot be considered simply as adjuncts to the process by which memories are set up. They also serve to maintain them. After removal of the lobes from an octopus discriminations that were well performed before cannot now be made. In this sense, therefore, the lobes form part of the memory store. Moreover, very curious situations arise in which the operated octopus appears to make no discriminations under some circumstances but behaves perfectly accurately in others. In particular, if food has recently been given, it will attack all moving figures. Yet if these are presented at times distinct from feeding no attacks are made at those from which shocks have been received.

This and many if not all the effects can be summed up by saying that in the absence of the vertical lobes the memories of the octopus are weak. This does not mean that they are short-lasting; on the contrary, such changes as are set up probably last for as long as in a normal animal. But in an animal without vertical lobes there are fewer opportunities for such 'learning changes' and they tend to be ineffective in the face of any circumstances that make accurate forecasting more difficult.

Valuable confirmation for this quantitative effect of the lobes comes from experiments in which differing amounts were removed, after which the accuracy of response was proportional to the amount remaining. This idea that memory capacity, and perhaps duration, in the nervous system depends on the number of neurons available has of course been emphasised repeatedly by Lashley, and there is much in common between his results with mammals and those with octopuses. In the present case we are able, as it were, to pinpoint the effect of the numerous small cells of the vertical lobe in raising or lowering the tendency to attack on any given occasion. The twenty-five million cells that they contain ensure the maintenance for a period of the order of two hours of a record or representation of the effect on the animal of a single appearance of a given situation. In the absence of these cells the record lasts only for a few minutes. With this knowledge we are assisted in looking for the underlying physiological changes. We know that we have to look for some after-effects of stimulation that are able to last for a period of hours, *provided that numerous cells are present*. There has been much investigation of the time constants of the processes that follow the passage of nerve impulses along single nerve fibres or through simple reflex chains. Perhaps we shall get further with the search for the nature of memory processes by recognising that they are the product of *aggregates* of cells. It is possible that within such aggregates the record is preserved at first in some dynamic form, perhaps involving repeated activity of sets of neurons. This would ultimately die away, but if it is reinforced by a further occurrence of similar situations, then ultimately some more permanent change occurs and 'fixes' the memory, perhaps by the enlargement of parts of the cells along the most-used pathways (Young, 1951).

However, it is obviously not possible here to discuss the great problem of the nature of memory records within the central nervous system. We are more concerned to use this particular case as an example of the fact that living organisms in

general possess this power so to adjust their bodily organisation that it shall enable them to survive if the future resembles the past. It would be premature to try to discuss the fundamental metabolic processes that confer this capacity. We can see, however, that at least in many cases selection among many 'possible' alternatives is involved. This power of 'adaptation' is a characteristic of systems that have many components, differing slightly from each other. Perhaps in the last analysis it is a consequence of the capacity of the carbon atom to produce a large range of slightly differing compounds. The point is that in order to investigate it we need to adopt methods that are capable of studying the performance over long periods of time of systems composed of aggregates of dissimilar entities. Such methods are bound to be different from those of traditional science, which have always sought to purify and simplify their materials. I have suggested that the analogy of the procedures of information theory may be helpful at least for introducing us to the nature of the problems we have to solve.

There is no doubt that the problems are difficult, but I hope that there are few biologists who would wish to avoid them. We each of us like to choose some relatively simple biological problem to which we can apply techniques partly worked out in other sciences. But, for the zoologist at least, there remains always the question of how to treat the whole organism, the whole species and the whole population of species distributed in space and time. I believe that consideration of these questions of memory and forecasting can help greatly with our thoughts about evolution and adaptation, showing us how to evaluate and compare the special characteristics by which each type of organism lives.

In some the instructions that control the course of life are mainly contained in the nucleus, so that each individual sets out with a limited range of responses, preselected of course by natural selection. The length of time for which such an individual can live will be limited by the accuracy of such forecasting, which will certainly not cover all possibilities, and the organism will sooner or later meet a situation in which it is destroyed. Life can be prolonged only by making new forecasts, either by the production of new individuals or by the adjustment of the forecasts by the setting up of memories within the individual. It is with this latter type of adaptation that we have been mainly concerned today. It may be interesting, however, in conclusion to consider the relationship of the two. Clearly, if the forecasts are correctly adjusted the organism is likely to live longer. And if it lives longer, then, as Medawar has shown, there will be an opportunity for genetic instructions that come into action more slowly to show their good or ill effects. Thus in species that have good brains there is an opportunity to select genes for long life. That there is some association of this sort is clear from the fact that men, elephants and horses are among the largest brained and longest-lived animals. Clearly the question of the size of the individual enters too. Rodents with relatively small brains are themselves small and live only short lives. Yet they are among the most successful mammals. What is the optimum mixture of instructions for each habitat—that obtained from the genes by rapid breeding, or that obtained by individually-acquired memories?

A large range of problems is opened here. They are of special interest to man in his present state in which the mean length of life is increasing. Is there any reason why the maximum should not also increase? It would seem at first that this is a genetic question, but that is outside my field. I am more concerned to emphasise the importance of the acquisition of adequate instructions by the individual. It is curious that our knowledge about how we store memories in the brain and elsewhere in the body is less than our knowledge of genetics or conventional physiology. Yet we are much more likely to be able to improve our performance by learning how to learn than in any other way. Indeed, should we not do well to direct much more attention to *control* of all living processes? If we learnt more of how the metabolism of each cell was regulated every minute, we should surely be forging weapons for

agriculture and medicine much more powerful than can be obtained only by the study of breeding. And this certainly applies to tissues other than the nervous system. We could probably all live longer and better lives if we knew how best to control our stomachs, our hearts, our muscles, and our livers—as well as our brains. Yet there is little scientific study of such questions—we leave them to quacks and moralists.

The appropriate scientific study can be undertaken in many ways. My own plea would be for the comparative method—to learn from the example of all the different sorts of animal that are before us. We have examples of animals with nervous systems at very different levels of organisation, each associated with a particular way of life. Surely there is a great future for study not *only* of the behaviour, as advocated by many ethologists, or *only* of the nervous system, but of the two together. We shall make advances when we can show how each type of brain enables the animal to meet the hazards of life in its natural environment. Is it too much to hope for the growth of a race of zoologists who have the ability and tastes appropriate to study both in the field and in the laboratory? They would certainly need long training and great industry and skill, but I prophesy that they would be most useful and happy men.

THE EVERLASTING HILLS[1]

ADDRESS BY PROF. D. L. LINTON

PRESIDENT OF SECTION E

'EVERLASTING' is a relative term. Everlasting flowers are merely flowers that last longer than usual. Everlasting friendship, at best only lifelong, may, when sworn between dictators, be perjured before the last echoes of celebration have died away. And Pope's 'everlasting yawn' that might 'confess the pains and penalties of idleness' is no more than a yawn too frequently repeated. Yet when in the story of the dying Jacob addressing his sons, the patriarch assures Joseph that in him the blessings of God shall prevail 'unto the utmost bound of the everlasting hills', an image of vast extension in both space and time is at once evoked. It was for the authors of the Old Testament story no more possible to conceive what lay beyond the 'utmost bound' of the hills than that there could be any time when the hills did not firmly bind the horizon.

But it is the business of science to overstep horizons and explore what lies beyond, and it was in the eighteenth century that the emerging science of geology—then more usually known as geognosy—became aware that it was exploring a past that reached back far beyond human generations. Particularly was this true of James Hutton. In his famous 'Theory of the Earth', which he read to the Royal Society of Edinburgh in the spring of 1785, he observed:

> 'The Mosaic history places this beginning of man at no great distance, and there has not been found in natural history any document by which a high antiquity might be attributed to the human race. But this is not the case with regard to the inferior species of animals, particularly those which inhabit the ocean and its shores. We find, in natural history, monuments which prove that those animals had long existed; and we thus procure a measure for the computation of a period of time extremely remote.'

With this enhanced vision of the extent of geological time Hutton saw clearly the mutability of what, to everyday sight, is everlasting. Believing that, under the Divine will, this world is a 'machine of peculiar construction . . . adapted to a certain end', he argued that there could be no plant life without soil, and hence that 'the surface of the land is made by nature to decay'. The evidences of that decay were to be seen on all sides. 'The heights of the land are thus levelled with the shores: our fertile plains are formed from the ruins of the mountains.' His 'Theory of the Earth' was rightly so called, for it was, in his own words, 'an investigation into the laws observable in the composition, dissolution and restoration of land upon the globe'.

From Hutton's views stem the essentials of our modern notions of the growth and decay of mountain ranges—their erection by the hypogenic, and destruction by the epigenic forces acting upon the crust. But not many in Hutton's day were prepared to contemplate with him 'the immense time necessarily implied for this total destruction of the land' and a century was to elapse before the Americans Powell, Dutton and Davis began to formulate the Huttonian conceptions in terms that permitted their application to the evolution of particular landscapes. To J. W. Powell we owe the concept of a 'base-level of erosion' below which degradation of the lands cannot go; to Clarence Dutton the emphasis on the equivalence of erosion and sedimentation as the 'debit and credit sides of one system of transactions' with its implied consequences for that state of crustal balance for which he coined the

[1] Address delivered at the Dublin Meeting of the British Association on September 6, 1957.

term 'isostasy'; and to W. M. Davis the invaluable idea that landscapes evolve through a recognisable *sequence* of stages, just as a child must become adolescent before he can become a man, and a man mature before he becomes old. The whole sequence he termed the cycle of erosion (originally the geographical cycle), and its penultimate stage, an actual land surface closely approximating to Powell's conception of base-level, he called a peneplain. In the present century Walther Penck and Lester King have put forward views differing from those of Davis as to the manner in which the cycle progresses toward its end and have introduced terms to embody their ideas. These afford important additional working hypotheses to be applied to the investigation of actual land-forms. But they have in no sense replaced Davis's fundamental concept of a sequence of forms leading to a base-levelled surface: they have merely provided alternative terms in the sequence.

The growth of ideas in this country about our own hills and mountains proceeded on different lines. Ramsay in Wales in 1846, J. B. Jukes in Southern Ireland in 1862, Archibald Geikie in Scotland in 1865, and W. Topley in South-East England in 1875, all regarded the visible landscape as the product of two processes—first, the cutting of a 'plane of marine denudation', and second, the excavation below that surface of the existing valleys. In attempting to put a date to these successive stages Geikie in 1901 made use of the Tertiary dykes that cross many of the Highland glens as evidence that when those dykes were intruded the glens had no existence. From this he concluded 'how prodigious the denudation must have been over the whole Highland region since older Tertiary time'. Yet Geikie's 'prodigious denudation' comprised only the excavation of the valleys we now see; the cutting of the marine plane represented by the Highland summits he relegated to the Palaeozoic era. He shared the view expressed by Lord Avebury in 1902 that 'the antiquity of the Alps and the Himalayas is little compared to that of our Scotch and Welsh mountains, which indeed are so low because they are so old'. To British geologists at the opening of the present century the idea of a major mountain range being reduced to the condition for which Davis had coined the term peneplain in much less than a major fraction of post-Cambrian time must have seemed unlikely indeed.

Since then work in other parts of the world has made it abundantly clear that the carving and even the destruction of mountain ranges is, by the geological time-scale, a relatively rapid process. Thousands of feet of rock can, in fact, be removed in a period which is only a fraction of Tertiary time. A good instance of this is afforded by the mountains of the north-eastern part of the South Island of New Zealand. The main range here is the Kaikoura, culminating in the peak of Tapuaenuku, 9,460 feet; parallel to it the Seaward Kaikoura rise directly from the sea-coast to heights of 6,000–7,000 feet; between the two ranges is a great depression occupied by the middle section of the Clarence river. The rocks that form both these ranges were powerfully deformed in Mesozoic times and it was the accepted view of New Zealand geologists at the opening of the century that the mountains visible today were elevated by the Mesozoic folding and have survived as mountains ever since. The middle Clarence valley, however, is a fault-angle trough (1) in which some 12,000 feet of Cretaceo-Tertiary sediments are preserved resting with strong unconformity upon the rocks deformed by the Mesozoic orogeny. Since there is reason to believe that these cover rocks once extended over the whole area concerned, it is clear that they too must have been involved in whatever movements raised the crust-blocks from which the Kaikoura and Seaward Kaikoura ranges have been carved. The newest of these cover rocks is a conglomerate series which contains abundant pebbles derived from the older formations of the cover; these were being eroded from the rising blocks while the conglomerates were being deposited in the intervening trough. But the conglomerate series itself, which is spoken of as post-Miocene, is involved in the tectonic disturbances and cut-off

upwards by the great reversed fault that bounds the Kaikoura mass on this side. The elevation of this mass is regarded as having occurred during Pliocene times, and while it was going on, and since it ceased, the whole of the cover rocks have been stripped from the original Kaikoura and Seaward Kaikoura blocks and the present bold mountain forms have been carved out of the resistant rocks of the undermass. Here is eloquent witness that in a region of high initial relief and considerable precipitation (50 to 70 inches a year today) thousands of feet of rock can be removed and entirely new landforms produced in only a fraction of Tertiary time.

Such an inference is strongly supported by the implications of the great unconformities of the geological record, and it is therefore pertinent to refer to three well-known British cases—the sub-Eocene unconformity in South-East England, the sub-Permian unconformity in Devon, and the post-Caledonian unconformity in the Scottish Highlands.

In the first case the surface of unconformity, apart from the tectonic deformation it has suffered in common with the Eocene deposits that rest upon it, is very nearly plane. Portions of it, stripped of their cover, are widely preserved as visible elements in the landscape. This surface, which has been interpreted as a sub-aerial peneplain trimmed by the waves of the Landenian sea that ultimately submerged it, is cut across the several zones of the underlying Chalk. Professor Wooldridge and myself twenty years ago mapped the outcrops of these zones on this sub-Eocene surface (2) and were able to show that, while in Suffolk and the Hampshire Basin the Chalk remains in almost its full thickness, in the neighbourhood of London the higher zones to a thickness of some 900 feet have been removed, and that in the North Downs and Chilterns this figure increases away from London and probably amounted to 1,500 feet in the Central Weald and Southern Midlands. In other words, after the deposition of the youngest English Chalk, South-East England was quite strongly deformed, the Central Weald and Southern Midlands standing at least 1,500 feet above the sea-level of the time. Following this upwarping a cycle of erosion ran its course and pared this landscape down to base-level. In Denmark during the same period the limestones of the Maastrichtian and Danian stages of the uppermost Cretaceous and the glauconitic sands of the Montian stage of the lowest Eocene were being laid down. Stratigraphically this seems a small enough equivalent for the base-levelling of 1,500 feet of rock, and even though the newly-consolidated Chalk may have been relatively soft, we are led to an enlarged appreciation of both the rapidity of erosion, and the duration of the pre-Eocene interval.

In Devonshire, Carboniferous sedimentation ceased in Lower Coal Measure times when the Hercynian orogeny raised the region above sea-level. Even before this happened, the lithology of the Culm, its intra-formational unconformities and pebble beds suggest that it accumulated like the Alpine Flysch on the flanks of rising folds that were being rapidly eroded. There is no local representative of the Upper Westphalian and Stephanian stages, and when sedimentation is resumed with the Permian rocks these rest with complete indifference on all the elements of the folded belt from North Devon to South, and from the Lower Devonian to the Upper Culm. Erosion had already gone far to reduce the Hercynian ranges, but, to judge by the coarseness of the basal Permian briccias and conglomerates, considerable relief still remained. These beds pass upwards into sandstones and marls which tell of a reduction of relief that may possibly have amounted to peneplanation (or pediplanation) of whatever mountain remnants survived above the heavily aggraded desert floor. This cycle was brought to an end by the rejuvenation marked by the Budleigh Salterton Pebble Bed.

The evidence of the destruction of the Caledonian mountains in Scotland is even more telling. Their elevation is pretty certainly indicated by the way in which the

sandstones and mudstones which constitute the uppermost Silurian near Stonehaven are succeeded conformably by an immense thickness of boulder conglomerates. Similar conglomerates constitute the lowest visible rocks of the Lower Old Red Sandstone throughout Strathmore but are soon succeeded by great thicknesses of volcanic lavas. Now it is a staggering fact that in Argyllshire the lavas of this same volcanic episode rest with violent unconformity upon the eroded outcrops of the mica schists, limestones, quartzites and epidiorites of the Dalradian sequence. Already the folds of the Argyllshire Caledonides had been laid open to the core and the lavas poured out across their worn-down outcrops. In Strathmore erosion was soon at work on the volcanoes and lava-covered mountains, and great thicknesses of sandstones and lava-conglomerates resulted. These give way to conglomerates with pebbles predominantly of quartzite and gneiss showing that the lavas had been largely stripped away and the underlying Highland rocks revealed again. Finally the Strathmore sequence is completed by some thousands of feet of sandstones and red mudstones. This drastic reduction in the grain size of the sedimentary material argues a corresponding reduction in the relief of the land surface possibly amounting to peneplanation of the Southern Grampians. By this time the mountains further north had been eroded to a depth vastly below their original surface, for the plutonic granite of Foyers, beside Loch Ness, had been not merely unroofed but opened to its deep interior before the conglomerates of the *Middle* Old Red Sandstone were laid down across its worn-down surface. These conglomerates, like the Triassic conglomerates of East Devon, no doubt mark the effects of renewed earth-movement and the beginning of a new erosion cycle, and it is reasonable to suggest that this was the same cycle that removed the *Lower* Old Red Sandstone lavas and conglomerates of the whole of the southern limb of the Ochil anticline in Kinross before the deposition of the *Upper* Old Red Sandstone there.

The facts and inferences that we have cited about Tertiary erosion in England and New Zealand, and about the destruction of our Caledonian and Hercynian mountains, may best be brought into some common relationship if we make use of the absolute chronology, based on the amount of radioactive change in suitable igneous rocks, which has been developed by geochemists in the last thirty years. Although we cannot by this means obtain any direct measures of the intervals of time that interest us, we can make some reasonable estimates within the framework of the larger spans of geological time that the geochemists have mapped out. It is now widely accepted that the whole of post-Cretaceous time occupied about seventy million years, and that the Miocene period began about thirty million years ago. On this basis we may infer that the time that has elapsed since the Kaikoura orogeny may be about fifteen million years, and that the interval of time represented by the sub-Eocene unconformity in South-East England was less rather than more than ten million years. Similarly, if we interpolate between the radioactively fixed landmarks of Palaeozoic time it does not seem possible to allow more than twelve or fifteen million years for the accumulation of the Lower Old Red Sandstone. The interval represented by the sub-Permian unconformity of Devon may be only fifteen or twenty million years, but the Permian period during which peneplanation may have been completed was possibly twenty-five to thirty million years long.

From these estimates it would appear that base-levelling can be completed in areas of modest elevation (1,000–1,500 feet) and soft rocks in less than ten million years: in fifteen millions vast quantities of material can be removed from high mountains like the Kaikoura and (remembering the unroofing of the Foyers granite) the Caledonian Grampians: in thirty million years the Hercynian mountains of Devon were possibly base-levelled. No doubt erosion cycles are not all of one length any more than human life-spans. But we have here at least a clue to the order of magnitude involved, and I think we shall not adopt a misleading estimate if we say that on a continental margin an erosion cycle can be carried to peneplanation in

twenty to forty million years according to the magnitude of the initial relief.[1] If this be so, it will be clear, and I would like to underline the point, that the seventy million years of post-Cretaceous time in Great Britain and Ireland have been ample for two erosion cycles to proceed to peneplanation across hard and soft rocks alike. In the face of such a conclusion it seems quite unlikely that present British landscapes will display relics of any pre-Tertiary erosion surfaces unless these have been exhumed during Tertiary times.

Before we embark upon the task of assigning the events which have produced our existing hills and mountains to some part or other of the last seventy million years, we have a preliminary question to ask and answer. In glaciated Britain and Ireland, how far are we looking at the forms produced by Tertiary erosion and how far have the hills we see been shaped by ice?

When I began work in Scotland in 1929 the views held there about the extent to which glacial erosion had modified the landscape were vague and a little contradictory. On the one hand, following the views of both Geikies, it was commonly accepted that hills of smooth outline—and such hills are widespread in both the north-east and south-east of the country—had been rounded off by the passage of ice over them at glacial maximum. This view was noted by Thomas Hay as that of 'the Scottish geologists' as late as 1944, although it had been specifically refuted by E. B. Bailey in 1935. Concurrently with this view, on the other hand, went a considerable reluctance to accept the possibility of significant valley deepening by ice. This, of course, was a great retrogression of opinion from that held in the late nineteenth century by James Geikie, and was no doubt in part the result of the views of J. W. Gregory on the origin of fiords, though these were strongly rebutted by Flett in 1927.

In the course of the next ten years my own fieldwork convinced me that glacial erosion in Scotland has been both more and less than was currently supposed. On the one hand are the upland landscapes of the east of Scotland in which V-shaped valleys with overlapping spurs and branching patterns are separated by zigzag divides showing the forms of maturity or the remnants of base-levelled surfaces. Though drift deposits and erratics show that these landscapes have been visited by ice, the forms of glacial erosion are rare, and where corries occur they are bitten into the smooth topography of maturity like chalk quarries into the English downlands. The topography is manifestly earlier than the corries and has evidently suffered little from glacial erosion. At intervals from the Cheviot to Caithness, where suitable massively jointed rocks occur, tors are found on the ridges and spurs, and since these are certainly not of glacial or post-glacial origin, they must have survived a glaciation in some way from inter-glacial or pre-glacial times. Of cognate origin with the tors are the exposures of deeply rotted rock, in some cases buried by glacial deposits, which testify to a period of profound chemical weathering older than the drift. Collectively the evidence suggests, since the forms are older than the corries and the rock-rotting than the drifts, that we have in these areas the survival of the forms and features of the pre-glacial topography, overpassed by the ice it is true, but very little modified by it. Such a conclusion is entirely in line with the recognition of a 'pre-glacial surface' by W. M. Davis in Snowdonia, and by J. E. Marr and other workers in the English Lake District. For all such areas our question is answered: the forms we see are so little modified by ice that we may hope to interpret them in terms of Tertiary physiographic history. And the success that has already attended some efforts in this direction is a clear indication that this conclusion is well founded.

On the other hand, continued fieldwork in the west of Scotland progressively

[1] In continental interiors the term base-levelling can hardly have the same meaning that it has at the continental margins and such an estimate as this is probably less helpful than one couched, as is a recent one by Professor Lester King, in terms of the rate of encroachment of the forms of a new cycle upon those of an older cycle that is still current inland.

convinced me that there the country has suffered profound modification by the ice. All the forms of glacial erosion—corries, troughs and ice-moulded hills, plateaux and lowlands—are displayed in the west in much greater abundance, in forms of larger size and more mature aspect and with systematically different altitudinal relationships when compared with their counterparts further east. I have recently attempted a systematic discussion of these facts elsewhere (3), and it may perhaps suffice if I here draw only the general conclusion, namely that these differences between east and west in the incidence of glacial erosion in Scotland are climatic in origin and reflect a distribution of precipitation in Pleistocene times in which the west was, as it is today, much wetter than the east. In fact, in an area in Western Scotland from Loch Maree to Loch Lomond, where precipitation today exceeds 100 inches a year, it is probably safe to say that no sizeable relic of the pre-glacial topography remains. Of hardly any other areas of the British Isles is this true, and of those in which this condition is most nearly approached—the mountains of Donegal, Mayo, Galloway, the English Lakes and North-West Wales—precipitation also approaches this degree of excess.

If we turn our attention to Southern Britain and Ireland we note also a progressive southward diminution in the incidence of glacial erosion which doubtless reflects a southward rise of the Pleistocene snowline with increasing temperature. In the extreme south the snowline passed above the uplands of Devon and Cornwall altogether. In these areas throughout the glacial episodes, and in most of Wales, the Midlands and Southern Pennines during the last glaciation, periglacial climatic conditions prevailed, and much rock waste was redistributed and a good deal more may have been produced by freeze-thaw action. Under periglacial conditions some pre-existing landforms were certainly modified, and the transformation of pre-glacial valley heads into 'nivation hollows' has often been mentioned as having occurred in Southern Britain though never demonstrated. But it is doubtful if any substantial change of hill or mountain form was brought about in the periglacial areas.

We may here draw our second main conclusion. In a large area in the Western Highlands of Scotland and in smaller areas elsewhere the visible mountain scenery is almost entirely or entirely the result of glacial erosion and is the grandest mountain scenery that our islands have to show. Ice work has been responsible not merely for the minor features of the mountains, but for their whole shape and style and their separation from each other, and in such areas as the Cuillins of Skye or Langdale in Westmorland, the whole visible landscape has been sculptured to its present form within, essentially, the last half-million years. Elsewhere in Britain and Ireland much survives from older, possibly much older, periods, and the question arises as we look at these essentially pre-glacial hill and mountain masses—in what part of Tertiary time did they receive their pattern and form?

In our attempts to answer this question two complementary lines of attack are open to us. Using the first we may recognise in the valley benches and upland plains of our hill masses surfaces produced in relation to former base-levels and by this means construct a denudation chronology of former partial cycles of erosion. This method is well known, and a convincing synthesis based upon it was offered to this Section in 1950 by its then President, Professor Wooldridge. The second mode of attack seeks to interpret the drainage pattern of a region in terms of major events in its physiographic history. Eliminating from the pattern all those elements that can best be explained as having arisen along favouring geological structures and all features that have resulted from the many forms of glacial interference, one is left with the consequent elements of the system. Their pattern and its relation to the geological structure of the region provides a basis for an inference as to the circumstances in which the system was initiated. There is a tendency for these two methods to be applied in rather contrasted ways. Studies of denudation chronology based on field recognition of the remnants of base-levelled surfaces are inevitably local.

Generalisations about drainage patterns have in some cases been based partly or largely on map study of large areas. Modern work, however, increasingly recognises the need to employ both techniques in all cases. This is well exemplified by Dr. Eric Brown's recent and manful attempt to grapple with nothing less than the denudation chronology and drainage evolution of all Wales (4). In a more limited field my own conclusions about the initial drainage patterns of the Tweed (5), Devon and Earn systems (6) in Scotland were all based on the recognition in the field of early high-level valleys of these rivers. The same is true of the account of the Welsh Dee which I gave to this Section in 1950 (7): that account has been greatly enhanced by the detailed study of the denudation chronology of the area since made by Messrs. Wilkinson and Gregory (8).

Now both sorts of evidence, that of drainage evolution and that of denudation chronology, are susceptible of two contrasted interpretations. Base-levelled surfaces may be of marine or sub-aerial origin. If they are the former, then the valleys that dissect them must have been cut by single-cycle streams initiated on the uplifted marine platforms in the way that Professor Wooldridge and I have postulated for much of the drainage of South-East England. If the surfaces are sub-aerial the streams have had a history extending through two (or possibly more) cycles. For coastal areas and low altitudes there is an obvious presumption in favour of the marine hypothesis, but for inland and upland areas any presumption is, perhaps, rather the other way. Several British workers, however, have claimed to recognise modified wave-cut platforms on the borders of our upland massifs. Professor Hollingworth in his well known statistical study of 1938 (9) recognised a series of four main and some subsidiary planation levels from 430 to 2,000 feet and regarded all of them as marine. Professor T. N. George in a study of the Southern Uplands (10) suggests that Professor Hollingworth's views are applicable there and mentions benches ranging from 600 to 2,300 feet. On the eastern flanks of the Yorkshire Pennines Dr. J. B. Sissons has mapped carefully spur features from which he infers a series of seven marine surfaces from 435 to 1,800 feet (11). In Exmoor, Professor W. G. V. Balchin has used similar techniques and recognises six marine surfaces between 280 and 1,225 feet (12). Collectively these findings imply a submergence of Britain, greater even than the glacial submergence of the late Victorians, at some date later than the mid-Tertiary folding of South-East England and followed by an intermittent but eustatic emergence. This is a challenging view, and perhaps the most important task before British geomorphologists today is to discover whether it has validity. It will therefore be appropriate if I spend a little time considering the implications of such a view.

First of all there is need for clarity in our conception of the state of the landscape when this great submergence occurred. Professor Hollingworth is explicit on this point and speaks of the submergence of 'a diversified topography of sub-aerial origin'. Professor George, when he speaks of 'platform residuals as centres of stream deployment', appears to envisage submergence of a landscape in which upland and lowland were already in existence. Dr. Sissons' work has the same implication, though he appears unaware of this and postulates a sub-aerial peneplain tilted towards the east and submerged possibly as the result of the tilting. As far as I can understand him the peneplain passed from the Pennine summits to those of the Moors and Wolds of Eastern Yorkshire and the tilting is invoked to account for the lower elevations of the latter. Now the cutting of a series of marine benches during the emergence of this tilted peneplain along the margins of a former North Sea is quite conceivable but not in the least what must be inferred from Dr. Sissons' evidence. His mapping leads him to postulate a series of marine benches along the flank of the Pennines against the Vale of York. That Vale must therefore have been present, far below the level of his presumed peneplain, for the sea to retire into stage by stage from 1,800 down to 435 feet, and since no known submarine agencies

could have excavated this vast hollow we must presume it to have been excavated sub-aerially by rain and rivers. In other words, Dr. Sissons' work itself implies the submergence of a landscape in which upland and lowland had already been created by differential erosion and in which subsequent vales on soft-rock outcrops were already in existence.

The difficulties raised by this concept of the deep submergence of a highly diversified landscape are severe, and I will briefly indicate those that chiefly deter me from accepting such a view.

1. First there is the absence of any vestige of the detritus that must have accumulated during the submergence in what are now inland situations like the Vale of York or Mid-Devon. Beach deposits or stream deltas corresponding to the higher shorelines might well have disappeared under the onslaughts of later erosion, but not so the fine-grained off-shore sediments of the major hollows. Nothing corresponding to the carse clays of the late-glacial 100-foot submergence of Central Scotland, or the St. Erth clays and sands usually associated with the 430-foot strandline of Cornwall have been reported. Yet such deposits should have accumulated in quantity and, preserved below base-level till the latest stages of the emergence, should have had excellent chances of survival.

2. Next there are the geophysical implications. If the hard-rock uplands of Britain had been already isolated by the excavation of soft-rock lowlands down to levels low enough to hold the seas that cut the 280, 320 or 430 foot seas, the following submergence to levels of 1,800, 2,000 or 2,300 feet means local depression of the crust by 1,500 or 2,000 feet. Such a foundering can hardly have occurred without subcrustal causes that might be expected to have igneous or other manifestations. Yet any such activity would have to go into reverse for the ensuing emergence. I find such notions as unlikely as the alternative of a world-wide rise of sea-level of 2,000 feet.

It should be emphasised here that these difficulties do not apply to all forms of the hypothesis of marine benching. They would not arise in the case of a stairway of benches cut upon the periphery of the British landmass, or, what might be the same thing, the border zone between a rising land and a tectonic downwarp. The crustal stresses involved would then be those that we have to disclose anyway to account for the present altitudes of our uplands. Difficulty arises when marine benches are claimed on the internal flanks of the uplands against interior lowlands since they imply deep submergence after those lowlands had been created by differential erosion.

3. Thirdly, the hypothesis is repeatedly in conflict with the evidence of the drainage pattern. Concordances have of course been noted by the authors mentioned between minor stream directions and those that could be ascribed to streams extending their courses across the postulated marine benches. Discordances are naturally not mentioned, but they exist, and are particularly disquieting since they concern what, on all other grounds, would seem to be the most significant streams of the areas concerned. An example from Exmoor must suffice to illustrate the point. South of Exmoor Professor Balchin's reconstructions show a series of nested gulfs reaching ESE. from Barnstaple on his Buckland, Georgeham and Instow strandlines. As the sea withdrew progressively from the first to the second and third a major stream should surely have followed. Instead the River Bray flows south across the axis of the depression and through the higher ground beyond to make the upstream junction with the Taw that Professor Wooldridge has emphasised as one of the significant clues to the early drainage pattern of the region (13). The drainage evidence in a word suggests that the Bray, like the Exe and Culm, is an original consequent stream antedating the visible landforms of the area, and the conflict with Professor Balchin's marine hypothesis could hardly be more complete. In the Pennines it is, to say the least, disappointing that Dr. Sissons' reconstructions fail entirely to offer any suggestion regarding the most puzzling rivers of the area,

the Derwent and Don, while in the Southern Uplands the implication of Professor George's remarks would appear to be that the major valley of the region—that of the eastward-flowing Tweed—was there before submergence occurred.

At this point I think we must consider where our argument is leading us. It is to be remembered that the work on Exmoor and the Eastern Pennines that I have cited was based, in each case, on detailed mapping at the scale 1/25,000 of actual spur flats over an area of some 900 square miles. It is not lightly that I question the results of work of this kind, especially since I know, in both cases, how thoroughly it was done. But since the results lead to conclusions so difficult to accept, I find it necessary to reconsider the fundamental assumptions on which the fieldwork was based. In both cases fieldwork followed, and was guided by, cartometric studies and altitudinal frequency analyses designed and intended, as are all such methods, to reveal the existence of erosion 'levels'. In the mapping particular attention was paid to the altitudes of 'flats' and 'rises' on interfluves, since by such features the remnants of marine benches and their inner margins might be recognised. The mapping, in fact, was guided by a preconception, and since no method of correlation of features was employed other than uniformity of altitude, it is almost inevitable that the mapped evidence should support the marine hypothesis. Neither author claims that *all* flats and *all* breaks of slope were mapped, yet nothing less can provide the evidence by which a choice between competing working hypotheses could be made. Such mapping of morphological features for its own sake is laborious but certainly necessary. It has been in progress in the Southern Pennines at the hands of a team of workers associated with the University of Sheffield for the past few years, but it is necessarily slow and the stage of interpreting the results lies in the future (14).

The objection here raised to the recognition of former erosion surfaces by mapping spur flats selected on a basis of altitudinal uniformity applies still more forcibly to statistical analyses of altimetric frequencies. Such methods can reveal only surfaces with a negligible range of altitude whether they are base-levelled plains just above sea-level or abrasion platforms just below. The graded valley floor that rises progressively into the uplands and the sea floor that descends progressively offshore alike fail to produce a peak on the frequency curve. It is a sobering thought that should we apply the altimetric frequency method to the London Basin there would be no risk that we should regard the summits of Shooters Hill, Kent (424 feet), Bushey Heath, Herts (506 feet), Headley Heath, Surrey (611 feet), and Penley Hollies, Bucks (664 feet) as belonging to a single marine stage. Yet all four carry relics of marine sands and shingles and were shown by Professor Wooldridge thirty years ago to have been part of the floor of the late-Pliocene Sea. With this in mind we would do well to take up again the query regarding altitudinal frequency curves put by Professor O. T. Jones in 1938 'whether the differences between maxima and adjoining minima are sufficiently great to be significant'.

If the considerations I have put before you have substance, it follows that in our endeavours to elucidate the stages by which erosion has fashioned the hills we must not put more weight on any type of evidence than it can properly sustain. Correlation by altitude alone, even over quite short distances, is to the geomorphologist as dangerous as correlation by lithology alone is to the stratigrapher. We must use in our reconstructions of the past nothing less than all the available evidence. Instead of considering only spur flats selected on the basis of altitude and comprising only a few per cent of the total area being investigated, the whole gamut of forms from valley slopes, through spur and interfluve flats to the upland plains and their surmounting residuals and divides needs to be reviewed. The unit for such studies is properly the drainage basin of a river, and an example on a modest scale that would in my view be immediately rewarding is the basin of the River Dovey above Machynlleth; here the valley slopes, intermediate spur flats, hill slopes and plateau

summits, are all conspicuously present in a basin, which though it opens seawards is partially enclosed, and the recent eastward extensions of the Dovey drainage by capture or glacial interference give rise to an opportunity to carry the valley chronology through the main watershed to the basins of the Vyrnwy and upper Severn. Our true upland plains, however, transgress the limits of drainage basins. Here our criteria for correlation must be found in the morphological character of the surface itself, the state of weathering of its rocks, the presence of any covering deposits, and the pedological development of its soils, together with the visible relations to higher and older features of the landscape and to the valley forms produced by younger partial cycles of erosion. If these criteria seem to you, as they do to me, somewhat subjective, that merely means that we must proceed with caution while we learn their use. In the long run I am sure they will serve us better in all our inland and upland regions than any correlations by altitude.

Here my review must end. Our hills are certainly not everlasting. Neither are they as old as was thought fifty years ago. We have learned that it is unlikely that any mountains can endure as such for more than about twenty million years unless they are raised up anew. All the hill and mountain features we see today belong to Tertiary times unless the present cycle of erosion is exhuming some earlier land surface like the sub-Eocene plane in the North Downs or the sub-Triassic hills of Charnwood Forest. Such features excepted, nothing we see in Britain or Ireland can be more than seventy million years old, though to be sure that is old enough. Most of it is much younger. In the areas of very heavy Pleistocene precipitation the whole of the mountain scenery has been produced in the last half-million years. Elsewhere the events of the Pleistocene period merely modified hill forms that have survived from late Tertiary times, and most of the upland country of Eastern Scotland, the Pennines, Central and South Wales, and South-Eastern Ireland is to be included here. The forms of these uplands bear witness to their evolution in stages that may carry us back ten, fifteen or twenty million years in time, but the work of elucidating those forms and stages is as yet hardly begun. It is possible that some of the work already done has been following a false scent, but where clues are few and problems large that may well be inevitable. The problems, however, are as fascinating as any that confront the scientific mind, and I doubt not that by addressing ourselves to them we are contributing to the enlargement of human experience.

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THE BALANCE-OF-PAYMENTS PROBLEM OF A EUROPEAN FREE-TRADE AREA.[1]

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I DO not propose in this lecture to enquire into the effect of the institution of a free-trade area in Western Europe upon productivity, standards of living, and so on. I propose only to raise some of the financial issues which are involved. Is it possible in Western Europe to combine free trade with full employment and balance-of-payments equilibrium?

Balance-of-payments problems as between the members of a European free-trade area can be avoided if each member takes steps to keep its own overall balance of payments in equilibrium. It will not matter if France, for example, is in deficit with the rest of the free-trade area provided she is in overall balance, i.e. in equal surplus with outside countries. She can then use the outside currencies which she earns to pay her debts within the free-trade area. And these outside currencies will be needed by the other members of the free-trade area; for if France is in deficit inside the free-trade area, some other member or members (say, Germany) must be in equal surplus within the free-trade area. But if Germany is also in overall balance, then the German surplus in the free-trade area must be matched by an equal German deficit with outside countries. If France has no overall deficit on her balance of payments, she can pay Germany with her earnings of outside currencies; and if Germany has no overall surplus on her balance of payments, she will need these outside currencies to pay her debts to the outside world.

This multilateral principle is well illustrated by the history of Benelux. Within Benelux the natural structure of trade and other transactions is such that the Netherlands is practically always in bilateral deficit with the Belgium–Luxembourg Economic Union. In the early post-war years the Dutch balance of payments was in overall deficit; and in these conditions the finance of her bilateral deficit with Belgium gave rise to problems which could be solved only by restricting Dutch imports from her partner. But from 1951 onwards the Dutch international payments regained an overall balance; and the Dutch were earning a surplus with outside countries which matched their deficit with the Belgium–Luxembourg Economic Union. The Dutch could pay the Belgians with the currencies of third countries.

Consideration of the payments problems of Benelux suggests that the maintenance of overall equilibrium in the balance of payments of each partner country is not merely a possible, but also the only acceptable, method of dealing with payments between the partners. Consider the alternative principle that each partner should keep its payments with the other members of the union (rather than with all the other countries of the world) in balance. This result could be achieved in Benelux without restrictions on intra-Benelux trade if there were a very great rise in Belgian prices and costs (brought about by inflation in Belgium or by an appreciation of the Belgian franc) so as to cause an increase in Belgian purchases from, and a decrease in Belgian sales to, the Dutch on a sufficient scale to bring the bilateral Belgian–Dutch balance of payments into equilibrium. But this would have threatened to put the Belgian overall balance of payments into serious deficit, since the rise in Belgian prices and costs would have increased Belgian imports from, and would have decreased her exports to, the rest of the world as well. Belgium would have had to impose strict controls over payments to other countries. Or if the Dutch–Belgian

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balance of payments were put into bilateral balance by such a deflation in the Netherlands as to reduce Dutch purchases from, and to increase Dutch sales to, Belgium to the required extent, then this would have caused a great decrease in Dutch imports from, and increase in Dutch exports to, the outside world as well. The Dutch would have to deal with a large undesired surplus in their overall balance of payments. In conditions in which the structure of trade and payments is such that some members of a free-trade area naturally have deficits with their partners matched by surpluses with outside countries, while other members are in the opposite situation, the principle that each member should be primarily concerned with its overall balance of payments is the only acceptable one.

Our first and basic principle is, therefore, that each member of the free-trade area should maintain equilibrium in its overall balance of payments. But before proceeding further I would like to make two comments on this general principle.

First, the maintenance of overall equilibrium in a country's balance of payments is nothing like so precise a criterion as might at first sight appear. Obviously, since some countries will naturally and properly be lending abroad or borrowing from abroad for ordinary commercial and developmental purposes, by equilibrium in the balance of payments we do not mean an equality between current payments and current receipts. We are concerned with the balance of all normal current and capital payments and receipts. Even so, we cannot make an exact equality between payments and receipts our criterion. A country which has very inadequate reserves of gold and dollars may properly aim at some surplus of normal receipts over payments which will enable her to bring her reserves to a reasonable level. We can only lay down a general principle that it is the duty of each member of the free-trade area to avoid a continuing deficit or surplus on its overall balance of payments which threatens to result in the unreasonable accumulation or loss of its reserves.

Second, the basic principle that each member of the European free-trade area should keep its own balance of payments in equilibrium will provide a solution for European balance-of-payments problems only if the outside currencies which some European countries will be earning are convertible into the outside currencies which other European countries will be needing to finance their net outside payments. In the Benelux example the outside currencies which the Dutch earn can be used to finance a Dutch debt to the Belgians only if they are convertible into the outside currencies which the Belgians need. In the immediate post-war years this was a very real problem; but it is reasonable to assume that the European countries are now in a sufficiently strong position to avoid it by insisting that they are paid for their exports to outside markets in acceptable outside currencies.

Let us then return to the main problem. By what means can each member of the free-trade area keep its overall balance of payments in equilibrium? There are at least five possible lines of approach to this problem which I shall call the liquidity approach, the gold-standard approach, the integration approach, the direct-control approach, and the exchange-rate approach.

By the *liquidity approach* I mean that steps might be taken to increase the availability of liquid reserves to the European countries which are in overall deficit so that they can thereby tide over temporary balance-of-payments difficulties. This cannot, of course, provide a full cure for lasting and permanent balance-of-payments deficits. But it can deal with temporary balance-of-payments problems and, above all, it can provide a buffer which will enable other measures for dealing with permanent difficulties to have time to work out their effects. Reserves of some European countries and particularly of the United Kingdom, the banker for the sterling area, are still lamentably low. An increase in liquidity is an essential ingredient in a satisfactory solution of our problem.

How can this best be achieved? One's thoughts naturally turn to the European Payments Union which provides a mechanism whereby, according to the credit

element in the monthly settlements, European countries with a surplus in their payments to other European countries automatically provide credit and so liquidity to those European countries which have a deficit in their payments with other European countries. Could the European liquidity problem be solved by a development of this European instrument whereby European surplus countries provide credits to European deficit countries?

There are two serious objections to reliance upon an E.P.U. type of mechanism.

In the first place there may well be times when the European free-trade area as a whole is in deficit with the outside world. This means that the overall deficits of the European deficit countries are greater than the overall surpluses of the European surplus countries. In such circumstances, it is impossible to cover the deficits of the deficit members by the surpluses of the surplus members. For this reason it would be much more satisfactory to proceed by means of a more 'universal' approach (such as an extension of drawing rights with the International Monetary Fund) which would give each European country, regardless of the position of its European partners, greater international liquidity to deal with its own overall balance-of-payments problems.

If, however, this more 'universal' approach is for any reason ruled out, it is possible to do something by methods whereby European surplus countries lend temporarily to European deficit countries. But there is now a second objection to an E.P.U. type of mechanism. My basic theme has been that if each member is in overall balance-of-payments equilibrium, then the settlement of payments within the free-trade area should be possible. Suppose that the Netherlands has a dollar surplus matched by a deficit with Belgium, while Belgium has a dollar deficit matched by its surplus with the Netherlands; it should be possible for the Dutch to pay the Belgians with dollars. But with the E.P.U. arrangements the Netherlands will continuously pile up reserves of gold and dollars, since their surplus is earned wholly in that form, while their deficit is payable through the E.P.U. partly in book credit. But Belgium will run out of gold and dollar reserves, since her dollar deficit must be financed wholly in that form while her surplus is with E.P.U. and will be financed partly by a book credit with E.P.U. This difficulty is avoided if settlements through E.P.U. are 100 per cent in gold; but in this case E.P.U. does not fulfil the function of providing additional credit by overall surplus to overall deficit members.

It is, of course, possible to devise methods for European surplus countries to lend to European deficit countries, which are not open to this objection. This might be done merely by arrangements between the exchange equalisation accounts or similar authorities of the members to the effect that the authorities of an overall surplus member would hold more of the currencies of an overall deficit member. Or it might be rather more institutionalised in the form of a European Monetary Fund into which all members paid certain amounts of their own currencies and possibly also of their reserves of gold and dollars and from which members with overall deficits were able to make temporary drawings. Or it might take the extreme form of the payment of all the gold and dollars of all the members into a common single pool, payments between members being financed by the transfer of claims on this pool, and payments to the outside world by drawings from the pool. This last system would, of course, have special implications for the United Kingdom since it would mean pooling with the Europeans the reserves held against the claims of the whole Sterling Area; and although there is nothing inherently illogical (indeed there is much positively desirable) in extending the payments mechanism to cover a wider region than the commercial free-trade area, the politicial difficulties involved are obviously very great.

In any case an increase in international liquidity for European countries is not a complete cure for our problem. What methods could be used to deal with persistent

balance-of-payments problems in the European free-trade area? Let us consider first what I have called the *gold-standard approach*.

By this I mean the application of the principles of the old gold standard as they are expounded in the text-books. Any member of the free-trade area which is in overall deficit will lose reserves of foreign exchange; it should allow this to lead to a restriction of the domestic supply of its money until its domestic money incomes, prices, and costs are so deflated that it has put its overall balance of payments into equilibrium by buying less imports and supplying more and cheaper products for export. Simultaneously any surplus member of the free-trade area should allow its receipt of foreign-exchange reserves to cause a monetary inflation domestically, which should lead to an increase in its imports and a decrease in its exports. This solution is dangerous. It requires that each European national government should devise its domestic monetary and budgetary policies essentially with regard to its balance-of-payments situation and with little or no thought for its domestic situation. Financial policies to prevent domestic inflations or to preserve full employment must be more or less abandoned.

This would mean that Germany, so long as she has an overall balance-of-payments surplus, must inflate her domestic money incomes, prices, and costs. But Germany with her memories of past hyper-inflation is very unwilling to do this. And, as recent events show, a surplus country like Germany is always able to restrain an inflation; she can, through restrictive domestic monetary and budgetary policies, offset the domestic inflationary effects of a balance-of-payments surplus and continue to pile up balances of gold and foreign currencies. But this means that a potentially deficit member of the free-trade area like France might have to make an undue share of the adjustment by a domestic deflation of its money incomes, prices, and costs; for it cannot indefinitely postpone the adjustment, since its stock of gold and foreign currencies is limited and exhaustible. It might have to abandon a domestic financial policy of expansion for full employment just at the time that the structural adjustment of its industries due to the removal of trade barriers within the free-trade area may be causing some redundancy of labour in its less efficient industries, so that it was especially desirable to have a domestic background of financial expansion to ease the development of its more efficient industries. Governments are nowadays so wedded (and, in my opinion, rightly so wedded) to the idea that it is one of their duties to preserve full employment that the probable outcome of this solution would in fact be the breakdown of the free-trade area agreements.

Many who accept these criticisms of the gold-standard approach may ask whether we could not get rid of payments difficulties at least within the free-trade area by combining with the building of a free-trade area in Western Europe the integration of European financial arrangements so as to make it as easy for a Frenchman to pay a German within Europe as it is for a Welshman to pay an Englishman within the United Kingdom.

The logic of this *integration approach* is unassailable. But it is not, I think, always realised how far-reaching this proposal is. This we can best see by asking why it is that the adjustment of payments between England and Wales is so much easier than that between Germany and France. There are at least five elements to the answer.

In the first place, the fact that goods, labour, and capital can move freely between England and Wales makes adjustment easier. Suppose Wales is in economic difficulty. A deflation of prices and incomes in Wales relatively to prices and incomes in England will have more effect in inducing consumers to buy Welsh rather than English products and in inducing workers to work in England rather than Wales, because there are no restrictions on the movement of goods or workers from Wales to England. Moreover, any rise in interest rates in Wales caused by a scarcity of money capital due to the deflation of money supplies in Wales would have more

effect in attracting new capital funds from England because there are no barriers to the movement of capital within the United Kingdom. Indeed, the Welsh and the English share in common the London capital market. A complete common market for goods, labour, and capital throughout Western Europe would similarly make the mechanism of adjustment of payments easier. There is some truth in the contention that the gold-standard approach is less dangerous when it is applied to the payments between the members of an economic union than when it is applied to payments between national States which maintain impediments to the movement of goods and services between them.

But this is not the whole of the story. A second reason why the adjustment of payments between England and Wales is much easier than between Germany and France is because the United Kingdom has, while Western Europe has not, got a single common currency and banking system. If the Welsh balance of payments with England is £1 million in deficit, then this means that £1 million of currency or bank deposits passes from the ownership of Welshmen to that of Englishmen. This deflation of £1 m. in Wales and inflation of £1 m. in England is the end of the direct monetary adjustment. But if the French balance of payments with Germany is 1 million francs in deficit, then the French central bank loses 1 million francs of foreign-exchange reserves and the German central bank gains an equivalent amount of reserves. There are many reasons why a change of 1 million in a country's foreign-exchange reserves may cause a change of many millions in its total domestic supply of money. In particular, if France has only a small proportion of her total domestic supply of money covered by foreign-exchange reserves, she may have to deflate her domestic supply of money by many times any loss of her reserves, in order to prevent a dangerous fall in her foreign-exchange reserve ratio. In other words, the absence of a single money means that the abruptness and speed of adjustment in France and Germany may have to be much greater than in Wales and England. Much of this problem would, of course, be solved by sufficiently far-reaching arrangements for increasing international liquidity between the members of the European free-trade area. In the example just given, if France had foreign-exchange reserves equal to 100 per cent of her domestic money supply, then any given loss of reserves would no longer make it necessary for her to deflate her domestic money supply by many times that loss of reserves.

Thirdly, England and Wales have a single national government which by the adoption of a stricter or easier central-banking monetary policy or by running a budget surplus or deficit can pump monetary purchasing power out of or into the system as a whole. This means that there can be an effective anti-inflation or anti-deflation financial policy for the United Kingdom as a whole which can take into account the interest both of Wales and of England. Trouble with the Welsh balance of payments need not be intensified by a failure of monetary demand in England to expand sufficiently. This is a most far-reaching difference between interregional and international payments. In Europe at present it is the function of each national government by control of its central bank's monetary policy and through its own budgetary policy to prevent undesirable domestic inflations and deflations. Free trade combined with fixed exchange rates (the gold-standard approach) would prevent European governments from devising their domestic financial policy for the purpose of preserving domestic stability. But the prevention of widespread booms and slumps is an essential feature of modern government. To entrust this task to a supranational European authority would require a very far-reaching surrender of powers by the national governments to ensure a single central-bank policy and a single budgetary policy for Western Europe as a whole.

Fourthly, suppose that the balance-of-payments deficit of Wales is greater than the balance-of-payments surplus of England, so that the United Kingdom as a whole has a deficit with the outside world. There exist central authorities for the United

Kingdom which will then do something to correct the United Kingdom balance of payments. Restrictions on imports into the United Kingdom as a whole would cause consumers in the surplus area, England, to purchase from the deficit area, Wales, goods which could no longer be procured from abroad. Similarly, a depreciation of the pound would cause outside goods to rise in price relatively to goods inside the United Kingdom, and England would buy more from Wales and less from outside countries and would sell more to outside countries and less to Wales. This also is a very far-reaching point. A European supranational authority with extensive governmental powers would be needed to devise and administer a single programme of control over imports into the free-trade area from outside (we shall return to this problem at a later stage) and to decide upon any changes in the exchange-rate between all European currencies on the one hand and all outside currencies on the other.

Fifthly, if in the United Kingdom (in spite of the factors which we have just mentioned) some economic adjustment does bring concentrated depression in a single region (like South Wales in the nineteen-thirties), then the central government in London exists to take special measures to bring new investment and enterprise to South Wales and to help to move labour out of South Wales. No such supranational authority exists to take such 'special-area' action on the part of Western Europe as a whole for the promotion of economic development in, say, a particular region of Europe, like Southern Italy.

The integration approach thus involves—in addition to the formation of a common market for goods and for factors of production and the provision of much greater international liquidity for European monetary authorities—a very extensive range of powers for what would amount to a single European government. Such a government would have to be able to control central-bank monetary policy and governmental budgetary policy throughout Europe, to determine a single European commercial and exchange-rate policy *vis-à-vis* third countries, and to carry out an effective special-area policy for depressed regions in Europe.

This is in my opinion ultimately desirable; let us hope that it will prove ultimately practicable; but it is not a starter at the moment and it would be a great shame to sacrifice the present real political possibilities of building a commercial free-trade area to this ideal of simultaneous monetary and budgetary integration.

Let us consider next the possibility of dealing with a deficit in a country's balance of payments by imposing restrictions on imports from abroad or on payments to other countries. This *direct-control approach* is more desirable than sole reliance on the gold-standard approach with its potential threat to full employment; and it is more practicable than the integration approach with its very far-reaching political implications. But in so far as it involves a deficit member of the European free-trade area restricting imports from another member of that area, it cuts deeply into the idea of a true free-trade area. Its interference with the free-trade principle is not to be measured merely by the amount of restriction of imports which actually exists at any one moment of time. The mere knowledge that trade may be restricted in this way in the future will discourage the large-scale investments that may be necessary to build up localised specialised mass production for the whole European market. The mass production of motor-cars in Detroit in the United States involves the investment of huge sums of capital in plant and equipment in Detroit, which is undertaken because the producer knows for certain that the whole United States market will always be freely open to him.

If restrictions on trade between the members of a European free-trade area are to be avoided, it must, of course, be possible for traders to make payments for such purposes. This does not in itself imply the absence of all exchange controls over the currencies of the countries concerned. Thus any one member country, say France, could maintain strict exchange controls; every Frenchman who wanted a foreign

currency, say German marks to purchase goods from Germany, might be required to obtain the currency from a central French exchange-control authority or its agent; and every Frenchman who acquired any foreign currency, say German marks, from the sale of French products to Germany, might be required to surrender the currency to the central French exchange-control authority or its agent in exchange for francs. All that is necessary to ensure the free entry of German products into France is that the French exchange-control authority should in fact always grant a Frenchman's request for foreign currency if he can show that it is required to finance the import of German products into France.

But the problem is very different if it is intended that there should be freedom of capital movements between the member countries. Full freedom of movement of capital funds from France to Germany, for example, means that the Frenchman must be free to buy German marks for all purposes, capital as well as current. But suppose that the German exchange control differs from the French, the French forbidding and the German allowing the movement of capital funds into dollars. If the Frenchman is free to lend to the German, and the German is then free to lend to the American, it does not require great ingenuity on the part of financiers (who are not conspicuously lacking in that quality) to devise means whereby in effect the Frenchman is lending his money to the American by way of a German intermediary. The control of the movement of capital from France to the United States will become much less effective if there is freedom of movement of capital funds from France to Germany and the German mark is fully convertible. To make one member's control over exports of capital to countries outside the free-trade area effective, the exchange-control regulations of all the members must be harmonised, if freedom of capital movement within the free-trade area is to be maintained. This is a very difficult operation. If it should prove too difficult, then freedom of movement of capital funds cannot be allowed in Europe so long as some members are in heavy deficit (and need to control the outflow of capital at least to the outside world) while others are in strong surplus and wish to make their currencies convertible into outside currencies.

But let us return to the problem of trade controls. Is it possible for a member of the European free-trade area to deal with a deficit in its overall balance of payments by restricting imports from outside countries without restricting imports from the other members of the free-trade area?[1]

The use by the members of the European free-trade area on balance-of-payments grounds of restrictions on imports from third countries can be organised in either of two ways which I will call the *national* and the *supranational* methods respectively. By the national method I mean simply that each individual member of the free-trade area might restrict imports from outside countries into its own national market as an aid to the preservation of equilibrium in its own overall balance of payments. By the supranational method I mean that the members of the free-trade area should set up some joint authority which would determine the total amount of outside goods which could be imported into the free-trade area as a whole, using this instrument to preserve equilibrium in the balance of payments of the area as a whole with the outside world.

There can, I think, be little doubt that in the circumstances of the European free-trade area it is the national method which must be employed, at least until there is some further movement towards extensive European integration. There is great difficulty in the supranational method. Suppose that Germany is in overall surplus and France in overall deficit, but that France's deficit is greater than Germany's surplus and the area as a whole is in deficit. To get a single joint programme of control of imports into the area from outside, agreement has got to be reached on at

[1] I do not propose to consider on this occasion which, if any, of the systems of import control discussed below would require a special waiver under the General Agreement on Tariffs and Trade.

least three basic points. First, how severe should the total restrictions be ? Germany may want the problem solved mainly by deflation or depreciation by France with little common import restriction, and France may want it solved mainly by inflation or appreciation by Germany with much import restriction. With the supranational method this choice would have to be made by common agreement. Secondly, given the degree of total restriction, which particular imports should be restricted ? France may want severe restrictions on the import of wheat which she produces but not on chemicals which she does not produce; and Germany may want the opposite type of import programme. Thirdly, given the degree of restriction of each particular import, how many of the import licences should be given to German and how many to French importers ? If we may draw any conclusions from the history of the Benelux economic union, we can safely conclude that the formation of a common programme of restriction of imports from third countries is one of the most difficult things to achieve even when there are only two trading partners to be considered.

Let us then consider the case of a member of the European free-trade area which is in an overall balance-of-payments deficit and which is trying to deal with this by restricting imports from outside countries without restricting imports from the other members of the free-trade area. In these conditions too much reliance cannot be placed upon the weapon of import control. In any free-trade area comprising the main countries of Western Europe, each individual member would in fact purchase a large proportion of its total imports from other members of the area. In 1955 the United Kingdom purchased 28 per cent, Western Germany 51 per cent, France 33 per cent, Italy 46 per cent, the Belgium–Luxembourg Economic Union 59 per cent, and the Netherlands 59 per cent of its imports from other countries of Western Europe. Import restrictions which were confined to imports from other sources would thus be limited in their scope.

Moreover, the limitation of this weapon is rather greater than these crude figures suggest. For the imports of the typical Western European country from outside Europe consist very largely of raw materials and foodstuffs which are less easily dispensable than many manufactured goods which it imports from its Western European partners.

There is a further reason why restrictions on imports from outside countries imposed on what I have called the national as opposed to the supranational principle may in certain circumstances prove a rather weak weapon of control. Suppose that France restricts imports of United States products because she is in deficit, but that Germany does not do so because she is in surplus. Then if United States products can move freely from Germany to France, the French restrictions become very ineffective. Indeed, for the system to have any sense at all it must be open to France to restrict the import of United States produce whether it comes into France directly or *via* Germany. But even if this is done, French restrictions on some United States products (say, motor-cars) may mean that more such cars are imported into Germany, thereby releasing for export to France some German cars which would have been wanted in the German market if Germany had also been restricting the import of United States cars. The national method will, because of such substitutabilities, be somewhat less effective a device for causing a net reduction in total imports than the supranational method. France will have to rely rather more on disinflation or depreciation than would otherwise be the case, when France is in deficit and Germany in surplus.

But when all the members of the free-trade area are simultaneously in overall deficit, when—that is to say—the free-trade area is in deficit because the outside world is in surplus with all the individual members, then the national method will work very effectively. If Germany is restricting the import of United States cars because she (Germany) is in overall deficit, then the restrictions which France is putting on the import of United States cars will not be offset by the increased

import of German cars which are being replaced by United States cars in the German market. If and when developments in the outside world (and, of course, in particular in the United States) impose an almost universal strain on European balances of payments, then the national method will automatically regain its full effectiveness. I personally regard the fact that it will be rather an ineffective tool except in circumstances such as these as a strong recommendation of it.

Because of the difficulties involved in operating the supranational method even the six countries forming the full European customs union would probably have to operate their restrictions on imports from outside countries by the national method. They would then have to maintain a customs control at their common frontiers to prevent the products of outside countries from entering a member country with severe import restrictions indirectly *via* a member country with lax restrictions on the products of outside countries. But the experience of Benelux suggests that, unless they are successful not only in imposing a single tariff of duties on imports from outside countries, but also in unifying all domestic excise duties and turnover taxes and in removing all their domestic agricultural support policies, they will in any case have to maintain customs controls at their common frontier.

But whatever the members of the full customs union may do, the United Kingdom must employ the national method for her restrictions on imports from outside countries. She will wish as far as possible to avoid discriminating against the products of Commonwealth countries, particularly if they are members of the sterling area; but if she joined in any common European supranational import-control plan, she might be forced to restrict imports from sterling-area-commonwealth countries even though she had to admit similar European products without restriction.

Incidentally, because of this special relationship both to Europe and to the sterling-area countries, the United Kingdom will find restrictions on imports from outside countries a particularly weak weapon. The United Kingdom in 1955 purchased 28 per cent of her imports from Western Europe and 38 per cent from the Sterling Area, leaving only 34 per cent from other sources for restriction; and much of this 34 per cent consists of basic materials and foodstuffs which it would be undesirable to restrict.

There remains the *exchange-rate approach* to the problem. A simple process of elimination leads inevitably to the conclusion that if the European national governments are going to use monetary and budgetary policies for purposes of domestic stabilisation—if, for example, in their present situation of balance-of-payments surplus the German authorities are nevertheless going to use their monetary policy to prevent a domestic inflation—and if it is desired to avoid the use of quantitative import restrictions on trade within the free-trade area, a greater use of the weapon of exchange-rate variations will have to be made. Some re-alignment of exchange rates may be particularly necessary during the initial stages of building the free-trade area. The removal of trade barriers of varying degrees of severity and the consequential development of import and export trades of varying degrees of expansibility may leave some members in deficit and others in surplus. The correction of this initial structural disturbance may require some exchange-rate adjustments quite apart from any need for exchange-rate variations to maintain equilibrium when once it has been achieved. Members of the free-trade area with a persistent surplus in their overall balance-of-payments will have to be ready to appreciate, and those with a persistent deficit will have to be ready to depreciate, the foreign-exchange values of their currencies.

This question is a difficult and controversial one, and it is not possible to discuss it at length in this lecture.[1] There are, of course, serious disadvantages in variations

[1] For my own views on this subject, see J. E. Meade, 'The Case for Variable Exchange Rates'. *The Three Banks Review*, September 1955.

of exchange-rates. There is the possibility that antisocial speculative movements of funds will be generated by the expectation of depreciations. There is the danger that a depreciation by raising the price of imports and so the cost of living will itself cause a rise of wage rates which, by making exports more costly, will give rise to yet a further depreciation.

These dangers will be much less if the European governments adopt effective domestic measures to prevent domestic inflations as well as to prevent domestic deflations. Speculation against a currency is most serious and dangerous when speculators think that a depreciation may set in motion the vicious inflationary spiral of higher import prices, higher wage-rates, higher money costs, more depreciation, and so on without end. Exchange-rate variations are certainly not a substitute for sensible and effective domestic policies to prevent inflation; on the contrary, they can be expected to work only if they are accompanied by such domestic policies.

The governments of all the countries of Western Europe are nowadays technically and politically in a position to control the total money demand within their countries by means of suitable budgetary and banking policies. What is much less certain is their ability to cope with inflation arising on the side of costs. If the institutions for the fixing of wage rates are such that, so long as there is reasonably full employment, money wage rates are pushed up more quickly than output per head, the European governments will be faced by an unhappy dilemma: through their control over banking and budgetary policies either they must allow an increase in the total money demand for goods and services which will provide employment for all workers at the higher money wage rates (in which case prices will rise as rapidly as costs) or else they must restrict monetary demand so as to prevent domestic purchasers from offering higher prices for the country's output (in which case higher money costs are likely to lead to unemployment). Only if suitable wage-fixing arrangements can be devised, will the European governments be able to use their control over banking and budgetary policies in such a way as to combine full employment with sufficient stability of prices to reassure their own citizens and foreign holders of their money that it is not necessary perpetually to speculate against a further loss of value and depreciation of the national currency concerned.

This problem of control of inflation from the cost side as well as the demand side is perhaps the most important economic issue which now faces the governments of Europe. In present circumstances it can be tackled only on a national basis, since workers' organisations and wage-fixing methods and habits vary widely from country to country. There are sure to be some divergences in its treatment; and differences in annual percentage rates of change of prices and costs in different European countries, though very moderate in amount, could give rise to serious balance-of-payments problems as they accumulate at compound interest over a period of years. For this reason, if for no other, rates of exchange between the European currencies must be variable, if it is desired to avoid more or less permanent restrictions on imports from being used as the way of meeting a growing divergence in levels of prices and costs.

But while exchange-rate variations are a necessary weapon in the armoury, it is of the utmost importance that they should not be regarded merely as another name for exchange-rate depreciations. Strong currencies should be expected to go up as much as weak currencies to go down. In order to establish this principle, it is especially important to initiate the use of this weapon in Europe with the appreciation of a strong currency rather than with the depreciation of a weak currency. There should probably be some depreciation of the French franc; but first and foremost at this moment there should be a substantial appreciation of the German mark.

I think that my attitude to exchange-rate variations is very much like Sir Winston

Churchill's attitude to democracy, which he once described as the worst of all possible forms of government except the others. And I am reminded too that Mr. E. M. Forster once wrote a book entitled *Two Cheers for Democracy*, so I ask you to join with me in giving one cheer for a depreciation of the French franc and two cheers for an appreciation of the German mark.

I will conclude by outlining my own tentative conclusions on this difficult problem.

Full employment is more important than free trade for Europe; and financial policies to prevent booms and slumps must for some time remain primarily the function of the European national governments. In order to ensure that they could not be forced to abandon their full-employment policies on balance-of-payments grounds they should for the time being be able to restrict imports even from their partners in the free-trade area, until experience has shown that alternative balance-of-payments arrangements can be made to work. But such restrictions are a serious derogation of the principle of European free-trade; they should be used only as a very last resort; and their imposition and use should be subject to the close supervision of an appropriate European authority.

A workable positive arrangement which would enable full employment to be maintained without restrictions on intra-European trade could meanwhile be worked out on the basis of three main principles. First, the European national governments must carry out effective domestic stabilisation policies, the surplus countries putting the emphasis on the avoidance of deflation and the deficit countries on the avoidance of inflation. Secondly, the foreign-exchange values of the currencies of countries in a persistent balance-of-payments surplus must be appreciated and of those in a persistent balance-of-payments deficit must be depreciated. Thirdly, greater foreign-exchange reserves must be extended to the deficit countries to tide over the process of readjustment.

There are many ways of implementing these three general principles; but by way of illustration I will tentatively recommend one particular method.

As I have said, domestic monetary stabilisation would remain essentially the concern of the national governments; but something can be done to encourage suitable national action by international discussion and co-operation. Each member of the free-trade area should formally recognise that its partners had a legitimate interest in the successful stabilisation of its own domestic incomes, prices, and costs and, in particular, in the avoidance of deflations by surplus members and of inflations by deficit members. There should be some European institution like the Organisation for European Economic Co-operation at which the national governments would regularly consult each other about their domestic financial policies.

Against a background of domestic stability the exchange rates of the national currencies of the members of the free-trade area should be allowed to float in a more or less free foreign-exchange market. This suggestion is liable to raise up a picture of wildly fluctuating European currencies, many of them losing their value completely. If reasonable domestic policies for domestic stabilisation are applied, nothing could be more absurd; and unless such policies can be applied, no sensible balance-of-payments policies for a true free-trade area can be devised. If reasonable domestic policies for domestic stabilisation are applied, there will still be some moderate external disturbances or some moderate divergences in price and cost levels requiring adjustment. To meet these, exchange rates will float moderately upwards or downwards; and every encouragement should be given for the development of a free market in forward exchange, so that the moderate inconveniences and uncertainties for traders resulting from these moderate fluctuations in exchange-rates may be minimised.

In such circumstances there should be no essential difficulty for the countries of continental Europe in allowing their exchange rates to float. There would be more

difficulty in the case of sterling, which is a currency used extensively by traders in other countries, held in large amounts by residents in other countries, and backed by inadequate reserves. There are, therefore, much greater possibilities of speculation by non-resident holders against sterling than against other currencies. But this is a difference of degree rather than of kind. It means that it is more important for the United Kingdom to obtain greater reserves or a greater degree of international liquidity by one means or another; and it means that it is especially important for the United Kingdom to devise an effective domestic policy for the avoidance of inflation from the cost as well as from the demand sides. In such conditions the pound could be confidently expected to float up as often as to float down; and there is no reason why in such circumstances a floating pound should not continue to be used as an international currency.

But what would be the use of a country's international reserves of gold and foreign-exchange in such a system in which its currency was allowed freely to find its own level in the foreign-exchange markets? The foreign-exchange markets, though in general free, might be subject to the intervention of the exchange equalisation account or similar authority of each member country, the authority selling some of its reserves of foreign-exchange for its own domestic currency to mitigate what it considered an unreasonable speculation against its currency, or vice versa. Cushioned in this way by the use of a country's reserves, the necessary fluctuations in a country's exchange rate could be taken at a very moderate speed.

Such a system is, of course, open to misuse if the authority in one country attempts to manipulate the exchange rate between its currency and the currencies of its partners to obtain some commercial or other advantage which its balance-of-payments position does not really need. Partly to avoid this danger and partly to provide greater liquidity for European countries there might be instituted a European Monetary Fund into which each member would pay an amount of its own national currency. This fund would be under some form of independent 'supranational' management of a technical banking character; and its management would be empowered on its own initiative to buy and sell the currencies in the fund to ease the balance-of-payments adjustments of the members. This fund would also provide a forum at which the policies of the fund itself and of the national exchange equalisation funds could be continuously discussed and integrated. To avoid speculation such integration would, of course, have to take the form of secret discussions between the central monetary authorities of the member countries. Such a system would be capable of almost indefinite development. As an integrated Europe became more and more of a reality, so the member States could pay greater and greater sums of their own currencies into the European Fund and could start also to pay into it part of their reserves of gold and dollars as well, until finally the supranational fund had superseded the national exchange equalisation funds. And as their domestic financial policies became more and more harmonised and integrated, so smaller and smaller fluctuations in exchange-rates need be permitted, until finally the conditions for what I have called the integration approach to the balance-of-payments problems have been fully met and exchange-rate variations can be abandoned.

The proposals which I have just made are, of course, riddled with difficulties and imperfections. I put them forward only as a challenge to others to produce something which is simpler, but equally effective, for dealing with European balances of payments without preventing European free trade or destroying European full employment.

EDUCATION AND TRAINING FOR THE ENGINEERING INDUSTRY[1]

ADDRESS BY SIR DAVID ANDERSON

PRESIDENT OF SECTION G

The Importance of the Problem

When I was invited to become President of this Section, I had considerable hesitation about accepting. I felt that a practising engineer or an engineering professor would have been a more suitable choice. On the other hand, the progress and prosperity of the engineering industry do not depend only on the education and training given to the applied scientists and technologists at the top, but on the education and training given throughout. During my professional life I have been concerned at one time or another with the whole field of education and training from the level of skilled craftsmen up to postgraduate studies and research. I decided, therefore, that I might, after all, reasonably accept the invitation. The subject is one on which it is difficult to say anything new, yet it is a subject of perennial interest. Education is a process of which we have all had experience and, therefore, we are all entitled to have opinions about it. Since this address is to be discussed, I propose to give some facts, to express some opinions, and then to invite contributions from others.

Engineering is the most widespread and basic of our manufacturing industries. Not only is it a major industry in its own right, but practically every manufacturing industry depends on engineering for the design, operation, and maintenance of its plant and equipment. Our whole industrial prosperity is therefore linked with our ability to organise and maintain a highly efficient engineering industry. Our need in this respect is clamant, since we are, economically, the most vulnerable industrial country in the world. Our massive imports of food and raw materials can only be paid for by an equally massive volume of exports. We want to export fine chemicals not common salt, scientific instruments not steel billets, electronic equipment not copper wire, precision machine-tools not steam-hammers, the finest textiles not cheap fabrics, and so on. To establish and sustain this great volume of high-quality exports we must have an engineering industry first-class in every respect, and the basis for that is a sound system of education and training.

I am not going to spend time attempting to estimate the number of engineers and scientists required by the engineering industry. The figure is important, but an estimate has already been made jointly by the Ministry of Labour and National Service and the Advisory Council on Scientific Policy, and the conclusion was that the number should be doubled in the next ten or fifteen years, that is, it should be about 20,000 per annum, and this was stated as 'a minimum goal which needs to be achieved if the economy is to grow at an acceptable rate'. Although the figure is a minimum I think we must face the fact that it is not likely to be reached: the accelerating pace of economic and technical change is such that there is every indication of a shortage for many years to come. The diversity of activities and multiplicity of professions in our modern way of life are such that, at the higher levels of intelligence, there is simply not enough manpower to go round. In industry the most urgent need today is not for more scientific discoveries but for a wider and more intensive application of discoveries already made. Every scientific discovery

[1] Address delivered at the Dublin Meeting of the British Association on Friday, September, 6, 1957.

opens up a fan of potential applications, and the engineering industry will be able to absorb for a long time ahead all the recruits it is likely to get.

Although in this address I am concerned with education and training more than with potential numbers, it is interesting to glance in passing at what other countries are doing. Valid comparisons are difficult to draw, but from all the information available it seems that Russia, the United States, Western Germany, and Britain are producing technologists, mostly engineers, in the ratio of 4 : 2 : 2 : 1 per unit of population. The conditions in Russia and the United States are very different from those in Britain, but with Western Germany we have much in common. The population of the two countries is the same, we are both at the same stage of industrial development, we are both heavily dependent on exports, and I find it more than a little disturbing that they are producing twice as many technologists as we are. This may be a highly significant factor when the European Market comes into operation. Indeed, it may be significant now. Look at West Germany's record in the matter of gold and dollar reserves. In 1949 the reserve was 149 million dollars, and at the end of 1956 it was 6,223 millions. For Great Britain and the sterling area the reserve was 2,688 million dollars in 1949, and at the end of 1956 it was 2,133 millions *including* 561 millions just drawn from the International Monetary Fund. It is impossible to say to what extent these figures reflect industrial enterprise and efficiency but I find them profoundly disturbing.

At the lower levels of employment in the engineering industry there does not seem to be any manpower shortage. I think most firms can get an adequate number of apprentices and some can even exercise a considerable measure of choice. If this is so, recruitment to the middle cadre, the technicians, should also present no problem.

Categories Employed in the Engineering Industry

The various categories employed in the engineering industry can be placed under two broad headings: professional and sub-professional, and these in turn can be subdivided into the following groups:

Professional Groups

Research Scientists, who are concerned with fundamental discoveries and new knowledge. At one time research scientists were to be found only in the universities, where the atmosphere is favourable to disinterested research, but to an increasing extent they are finding a place in industry. Nowadays, much work in this field requires resources that only industry can provide.

Development Engineers and Designers, who take the discoveries of the research scientists and apply them to practical problems. They carry out applied research and experimental work in industry up to what might be termed the pilot-plant stage. The designers then work out the final design.

Production Engineers and Managers, who take over responsibility from the design stage and carry through all the processes of manufacture. They have to *make* the product. Since at this stage large numbers of workers are usually involved, management is a very important aspect of a production engineer's work. Indeed, management is becoming so important a function that it is sometimes exercised by persons not directly connected with production, e.g. personnel managers and labour managers.

These three groups are of full professional status. The classification is fairly arbitrary and a separation, as clearly defined as I have expressed it, would be found only in the largest firms. In smaller organisations the arrangement is more flexible. One individual usually exercises several functions. At one time he may be attending to design and development, at another to production, and at still another he may be doing his best to avert a strike. Nevertheless, whether the duties are performed

by one or more individuals, the functions of development, design, production, and management, all come into the picture, even in the small concern.

Below them come the sub-professional groups:

Sub-professional Groups

Technicians, who are men with some scientific training working as a rule under the general direction of technologists; they are frequently in charge of processes where there is some element of control through instrumentation. This group is growing both in numbers and importance and is now a characteristic feature of modern industry.

Supervisors and Foremen, who are in charge of shops or departments rather than processes. Their field may be less scientific, but is generally wider, than that of the technicians.

Skilled Workers, who operate machines and carry out skilled processes not yet mechanised. While this group is diminishing in number, being gradually replaced by automatic machines and technicians, it is still substantial, and the rate of diminution is slow.

I think all those employed on the technical side of the engineering industry can be placed in one or other of these groups. Someone might comment on the absence of draughtsmen, but I think they are covered; the upper ranks can appropriately be placed with the development engineers and designers and the lower ranks with the technicians. Before proceeding to discuss education and training for the different groups I want to touch on the parts played by technical colleges and universities in this field.

Relative Contributions of Technical Colleges and Universities to Education and Training

I have put technical colleges first because their contribution is numerically greater than that of the universities and because they make an important contribution at every level except that of the research scientist. Between the two world wars there was a remarkable development in the volume and standard of work in technical colleges. Post-war needs have accelerated this development and a new pattern for technical education is emerging in England. Hitherto nearly all institutions have been treated alike in a common 'technical college' group, but a certain difference of function is now being introduced. There is to be a small group of institutions called colleges of advanced technology, at the moment eight in number. As the name indicates these colleges are to concentrate on advanced work, mainly, but not exclusively, in the form of full-time sandwich courses. Next there is to be a group called regional colleges. These will generally serve a wider area than that of a single education authority. They may offer sandwich courses as well as a large volume of advanced part-time work. Next there are to be area colleges and local colleges: these will be concerned with technical studies at a lower level, and work of a non-vocational character.

In the past the development of courses of graduate standard in technical colleges has been handicapped by the lack of an award commanding national recognition like a university degree. The new Diploma in Technology established by the National Council for Technological Awards under the chairmanship of Lord Hives removes this handicap and a rapid expansion in the volume of advanced work in the major technical colleges may be anticipated. The diploma is to be equivalent in standard to a university degree and already there is a demand that the Council should establish a higher award comparable with a Ph.D.

There have been developments too in the universities. They have all increased their accommodation for science and technology in accordance with government policy and further expansion is being planned in selected centres.

With all these developments, both accomplished and planned, it is opportune to try to assess the relative contributions which technical colleges and universities are going to make in supplying the engineering industry with trained men.

The sub-professional group, comprising the skilled craftsmen, supervisors, and technicians, whose education is provided for in part-time courses, will be dealt with by the appropriate technical colleges: the universities have clearly no interest in this field. In the professional group the research scientist will remain a university product, but the development engineers and designers, and the production engineers and managers may be educated either in the universities or in the colleges of advanced technology or in some of the regional colleges. The university courses follow a uniform pattern of three years post-intermediate and lead to a degree. In the technical colleges there is some variety. The new sandwich courses leading to the Hives diploma will probably be of four, or perhaps five years' duration. Some colleges now offer, and some will continue to offer, courses leading to the London external degree and these may be on a full-time or part-time basis. Lastly there are the widespread part-time courses leading to a Higher National Certificate. Not all those obtaining a Higher National Certificate can be reckoned as of full professional status. The H.N.C. is a borderline qualification serving the lower ranks of the technologists group and the upper ranks of the technicians group. Probably about half those getting H.N.C.s will, with further study and experience, reach full professional status. With this general pattern in mind it is interesting to examine the present output of engineers of graduate standard from the universities and the technical colleges as shown by Table I. A second table gives the output of scientists for comparison.

TABLE I

Output of Professional Engineers

Note: The figures for first degrees and diplomas are those given in the U.G.C. Returns under 'technology'; the great majority of these will be engineers. The London figures all relate to engineering. The Higher National Diplomas are in mechanical and electrical engineering and the Higher National Certificates are in mechanical, electrical, civil, and production engineering. It has been assumed that 50 per cent of those getting H.N.C.s are of potential professional standard.

QUALIFICATION	SESSION					
	1950/51	1951/52	1952/53	1953/54	1954/55	1955/56
First degrees and diplomas (U.G.C. returns) .	3175	3145	2999	3009	2760	2871
London, internal degrees from institutions with recognised teachers	242	268	231	209	219	233
London external degrees	570	456	469	309	250	237
Higher National Diplomas	245	203	264	239	251	310
Higher National Certificates, 50 per cent of output	2598	2687	2769	2860	3041	3107
Total	6830	6759	6732	6626	6521	6758

These figures are profoundly disturbing, particularly those for engineers. Our chief industrial competitors are well ahead of us now in their output of engineers and there is clearly no hope of closing the gap with numbers which are virtually stationary. Admittedly, the campaign urging the importance of technological education and the programme to improve its facilities have not been long under way,

TABLE II

Output of Scientists

Note: The figures for first degrees and diplomas and the London figures are graduations listed under the Faculty of Science. The Higher National Certificates are in chemistry and physics. It has been assumed that 50 per cent of those getting H.N.C.s are of potential professional standard.

QUALIFICATION	SESSION					
	1950/51	1951/52	1952/53	1953/54	1954/55	1955/56
First degrees and diplomas (U.G.C. returns) .	4474	4709	4660	4568	4564	4444
London, internal degrees from institutions with recognised teachers	338	329	276	210	194	178
London external degrees	965	1112	984	909	710	720
Higher National Certificates, 50 per cent of output in chemistry and physics . .	223	268	320	367	446	520
Total	6000	6418	6240	6054	5914	5862

but even so too much should not be expected. The fact is that too many professions are chasing too few bodies, and other professions are conducting intensive recruiting campaigns. The chance of securing 20,000 graduates in science and engineering in the next ten years seems slender. There will undoubtedly be some increase in output both from the universities and the technical colleges, and I am fairly certain that the major increase is going to come from the technical colleges. These institutions are all linked with the great stream of part-time education, and it is in this stream that there is an appreciable reserve of good brain-power which could be picked out and transferred to full-time courses. It is doubtful if there is any appreciable reserve left in the grammar and secondary schools for direct recruitment at G.C.E. advanced level unless considerably more young people are going to show an interest in science.

Just after the war a question much debated was the relative roles which should be played in engineering education by the universities and the major technical colleges. Many held the view that the technical colleges should provide a more 'practical' type of course, leaving the more theoretical approach to the universities. Like many other generalisations, this one was arrived at by feeling rather than by analysis. Actually, when one gets down to hard facts and draws up courses and syllabuses to provide the type of man industry wants, it is perfectly clear that in undergraduate courses of equal length the content and approach can differ very little whatever institution is offering it. It is a case of doubling-up the supply rather than producing alternative types. The new pattern in technical education is in line with this view. The accommodation, equipment, staffing, and conditions of service in the colleges of advanced technology are to be comparable with those in the universities and there is to be the same emphasis on research. The courses are to have the same content of basic science, and hence the man graduating after a five-year sandwich course will be pretty much the same type as the three-year university graduate after he has done a two-year postgraduate apprenticeship. There are now two routes to the same goal and a substantially larger number may well elect to take the sandwich-course route. There is no reason why they should not do so; the choice depends not on intellectual ability but on personal taste and inclination. There will be many young men who will welcome the earlier entry into industry now that the end-result can be the same.

The colleges of advanced technology will undoubtedly press for the early establishment by the Hives Council of a higher award. When that comes about nothing would be more stimulating than to see a considerable two-way movement of graduates between the colleges and the universities for postgraduate study and research.

I have spent some time on this new pattern of technical education for it is a very important issue. The pattern is now set for at least the next twenty-five years and much is going to depend on the results. So far as the quality of education is concerned I have no doubts, but I am equally certain that the required output will not be obtained. Nothing must be left undone which would encourage young people to choose technology as a career, and in my opinion no single step would produce greater results than the conferment of degree-granting powers on some of our leading technological institutions. To draw again a comparison with our international competitors, we are the only industrial country in which the leading institutes of technology do not have full university status and in no other country is the output of engineers so small. I, personally, see a close connection between the two.

Some Problems of Courses and Teaching

A problem which received some attention before the war, and which has received a great deal more since, is that of introducing a measure of liberal studies into technological courses. There is admittedly a problem but I think the magnitude is often exaggerated. Fervent advocates of liberal studies talk as though scientific and technological courses had themselves no liberal value, and as though students encountered no liberalising influences outside their teaching institutions: neither is true.

What these advocates have in mind is that our students must realise that we live in a human world full of human problems and responsibilities and not in an isolated scientific world of controlled experiments and results. But no one these days can remain unaware of that fact? Students have never had more opportunities both as students and in their private lives for social and cultural contacts. Our newspapers, weeklies, cinemas, theatres, radio, and television, cannot leave anyone unaware of the human problems in modern society, and the technologist in industry is often right up against such problems in very intractable forms. It is not so much an awareness of the problems that is needed as help in dealing with them and perhaps we could do a little more in this direction. A fair amount is being done in corresponding courses in America, Germany, Switzerland and Russia where an appreciable content of liberal studies is often included. We could, I am sure, get over the usually alleged difficulty that there is no time. Engineering courses contain many laboratory periods and I imagine no great harm would be done if we stole a few half-hours from some of them.

Everyone will have his own ideas as to how he would use such periods. My own preference would be for some attempt to teach the writing of good English, some lectures on logic as an aid to clear thinking, something about the history of science and industry, something about psychology as it bears on our thinking processes and group reactions, and something about philosophy and values. Certainly every technological course would be the better for the inclusion of these and the job of getting the material across is not impossibly difficult. The B.B.C. Third Programme contains many brilliant examples of how such subjects can be attractively presented in a non-specialist way.

Another criticism of technological courses, whether conducted in universities or technical colleges, is that students are not being taught to think, but are simply being crammed with facts. In these days of overcrowded syllabuses there may be some measure of truth in the criticism, but, like that directed against the narrowness of technological courses, a good deal of this criticism is glib, uninformed, and

exaggerated. The burden of the complaint is that our students are expert only at applying formulae, whereas they should know how to analyse a problem and apply first principles. But that is precisely the approach which every serious teacher tries to inculcate. In this respect there is little failure, I am sure, on the part of the teachers.

Does the criticism not stem from the fact that very few people are good at thinking, which is a difficult and highly-skilled process? The first-class thinker is rare. Even though our students are drawn from the higher intelligence groups, the majority have only moderate thinking capacity and it is doubtful if any great improvement could be effected by changes in teaching methods. The capacity to think is probably an inherent quality of the mind, and is probably much influenced by the way knowledge is presented and acquired.

This criticism about an inability to think comes usually from an employer who is disappointed that a man's performance is not measuring up to his paper qualifications. It would help if academic institutions could classify their men better than they do at present; there would then be fewer disappointments. This might be done if we reduced our dependence on written examinations, and increased our oral examinations. An oral examination can detect the lucky 'shot-in-the-dark', which must often undeservedly score full marks in a written paper. Another way would be to make more use of projects and theses. These should involve some reading, with or without experimental work, but not necessarily research. This kind of test can display a student's ability far better than a written paper.

Another teaching problem is how best to deal with overcrowded syllabuses. The rate at which new knowledge is accumulating makes this a major issue and the job of the teacher is becoming progressively more difficult. I wonder if the teacher training colleges and university departments of education could be of help here? Much research has been done on the learning process and teaching methods at the infant and primary stages, but, so far as I know, nothing much has been done at the undergraduate level. Is it fantastic to suggest the application of some kind of 'work study' to mental processes in the same way that work study is now applied to physical processes with excellent results? Workshop operations change their character, but old methods persist until a scientific analysis shows the result to be an inefficient muddle. A string diagram of a worker's movements in an area of 500 square feet may disclose that he walks miles in the course of the day. When this is put right by work study the walking may be reduced to yards and the output go up many-fold. I have a shrewd suspicion that many of our mental processes are carried on at the same low level of efficiency and that similar improvements could be effected if we knew how to set about it. I have a feeling the problem might handsomely repay investigation by experts. If the Staff College for teachers, recommended in the Report of the Willis Jackson Committee, comes into being, and there is a great deal to be said for it, here is an excellent problem for study ready to hand.

More effective teaching apart, strict scrutiny should be exercised over all syllabuses. Engineering is particularly prone to heavy time-tables, far heavier than are found in the arts or even in the sciences. This is partly due to the variety of subjects which must be given a place in an engineering course and to the large amount of laboratory work associated with them. Although it must be conceded that some of this laboratory work corresponds to the essays written by an arts man, nevertheless, the total load is extremely heavy. I think the situation is getting worse, as engineering departments grow in size. The old all-round teacher who dealt with several branches of his subject is disappearing and is being replaced by a number of specialists. With large departments this is inevitable, but the old teacher had to compromise only with himself when it came to allotting time to each branch. Today the compromise is between competing specialists, each of whom wants all the time he can get. Great responsibility rests on heads of departments to see that enthusiasts are restrained and syllabuses kept within reasonable bounds.

Pressure in undergraduate courses may be eased a little by the courses of postgraduate study now being established in the universities. The support for these has so far been meagre, the counter-attractions of industry being so strong, but the new D.S.I.R. awards for postgraduate study should help. It is conceivable that some of the new colleges of advanced technology may play an important part in this postgraduate field. Particular colleges, because of their geographical location, might be associated with particular industries, and courses of postgraduate study might be planned jointly with the industry.

Management Studies in Engineering Education

To be efficient the engineering industry must have good management, because without good management the maximum return cannot be reaped from technological advance. Management studies should therefore have a place in engineering education. A common attitude to education for management is that 'managers are born and not made'. Fortunately, this attitude is changing. It stems, of course, from the fact that management is both a science and an art, a phrase often used without the relevance being made clear.

Broadly, a science is something which we 'know', and is a body of fairly precise and verifiable knowledge; an art is something which we 'do' and knowledge in the scientific sense may play no great part. Every human activity is a mixture of science and art, but with wide variation in the relative proportions. Mathematics, the purest of the sciences, is largely a matter of 'knowing'. Chemistry, another pure science, is also a matter of 'knowing', but with an art aspect in the experimental skill of the chemist. When we come to the applied sciences, such as engineering, the proportion of science and art, of 'knowing' and 'doing', has greatly changed. The main function of engineering is to make things, and, although there is much science in engineering, a great deal can be learned about the 'doing' aspect from sheer practical experience. When we come to management we have taken another step towards the art end of the science/art spectrum.

Now the further we move towards the art end of the science/art spectrum the more difficult the subject becomes to teach, because skill in an art is more difficult to impart than knowledge of a science; it needs practice, more than memory and understanding. It follows then that training for management must consist of two parts. First the acquisition of certain factual knowledge in subjects like industrial history, economics, psychology, industrial relations, costing, work study, and so on, and, second, opportunities for practice in the art. This means a co-operative effort between teaching institutions and industry, and of all branches of study, none requires closer co-operation. The materials which the engineer or metallurgist uses in the shops are the same as those he formerly used in the teaching laboratory, but the reactions of human nature on the shop floor have no counterpart in any classroom experience. If, therefore, we are going to raise the standard of management, industry must organise proper schemes of practical training.

There is a danger that industrialists will think too lightly of the efforts of academic institutions to teach management subjects, and will fail to reap benefits within their grasp. Industrialists know that teaching salaries are such that no college or university could ever compete with industry in the management field, but this is not so serious as it seems. A department of management studies is unlike any other department. Far less formal teaching is required and considerably more discussion. In science and technology the behaviour of things is predictable and can therefore be dealt with by the guidance of theory. Human reactions are unpredictable. There is infinite variety in human relations, no situation ever repeats itself exactly, hence practical experience can be a better guide than theory. One of the main functions of a department of management studies is to provide the surroundings and create the atmosphere in which intelligent discussion of experience can take place.

Management studies are commonly undertaken at the postgraduate stage, though they can have a considerable place in undergraduate courses of the type called in America 'industrial engineering'. I think the ideal arrangement at the postgraduate stage would be a planned series of short residential courses spaced throughout the earlier years of a man's professional life and I would emphasise the desirability of a series. In the hustle and bustle of industrial life there is just no time for reflective thought and unless opportunities are specifically provided, creative thinking about one's job goes by default.

Foremen and supervisors need such training as much as any other management group. I should like to see every man who was earmarked for promotion to foreman or supervisor being withdrawn from the shop floor and sent to a short residential course before taking up his higher appointment. Not only would this raise his status in the eyes of his fellow workers, but the training he received might make all the difference to his success in the new post.

Sub-Professional Groups

No survey of education and training for the engineering industry would be complete without reference to the sub-professional groups: supervisors, technicians, and skilled craftsmen. In these groups there does not appear to be any problem of shortage. Most engineering firms are able to recruit all the apprentices they need, and can frequently exercise a considerable measure of choice. Technicians may be recruited from senior apprentices or skilled craftsmen, or directly from school. Supervisors will generally be promoted from the ranks of the skilled craftsmen.

I think the time has come when British industry should make a serious examination of the apprenticeship system. A higher standard of technology needs to be backed up with better technicians and craftsmen. At the start of the century apprenticeship was much the same in Britain and in Continental countries, namely, a five-year period of practical training with some voluntary attendance at evening classes by a small minority. Today British and Continental practice is widely divergent. West Germany has established apprenticeship on a national basis, with a rigorous system of training, examination, and certification. All apprentices are indentured, and the indentures are registered with the local Chamber of Industry and Commerce. Throughout the apprenticeship there is compulsory day attendance at a vocational school for six to eight hours per week. The duration of the apprenticeship is only three to three and a half years, and an apprentice must pass a written, oral, and practical examination before he can be certified as a skilled craftsman. The Chambers of Industry and Commerce are responsible for organising the examinations, in which the employers and trade unions co-operate.

The system applies to commerce as well as industry, and in Western Germany, there are nearly six hundred occupations recognised as requiring a proper apprenticeship training. The problem of the small firm, unable itself to give a good all-round training, has been solved. Communal training establishments have been set up to give a basic training to apprentices from such firms. The work of these communal training establishments is highly regarded. There were about three thousand in operation before the war, and it is intended that the number should be increased. As an example of the scale on which apprentice training, examination, and certification is conducted it is interesting to know that in the City of Frankfort, population about 600,000 the Chamber of Industry and Commerce in 1954 examined 4,800 apprentices in 126 different trades. A full description of the German system is given in a booklet 'Gaining Skill' produced by the Birmingham Productivity Association.

If we look at France we find the same problem being tackled in a different way. An apprenticeship can be served, not only in industry, but also in an apprentice training centre. These centres are part of the educational system not the industrial

system, but employers and trade unions are closely concerned with their operation. There are nearly 1,000 such centres catering for 160,000 apprentices—98,000 boys and 62,000 girls—and the courses last three years. With this arrangement the apprenticeship is completed before entering industry. About 40 per cent of the apprentices in France are trained in this way, the remaining 60 per cent serving a traditional apprenticeship. The apprentices trained in the special centres form a kind of *corps d'élite* from whose ranks are drawn foremen and supervisors.

In Switzerland the system is somewhat similar to the German system. There is compulsory part-time day attendance with a system of examination and certification. The duration of the training period is three to three and a half years.

It is clear that apprenticeship training is now more thoroughly organised on the Continent than in this country. Far too many British firms give little attention to this problem and in the light of what is happening aboard it is disturbing to reflect that in Britain a youth can become a skilled craftsman without having attended a single class or passed a single examination in the whole five years of his apprenticeship. 'Serving time' is the only compulsory requirement, and there is not the slightest doubt that in many cases this time is served in a very slipshod fashion. Without necessarily copying either the French or German systems, we should certainly address ourselves to improving our own.

The technician group, which falls below professional status, does not present any serious educational problems. The group is increasing both in size and importance because the tendency today is to bring everything under control and reduce to a minimum the personal choice of an operator as to how he will do a job. This means that after the design stage is finished there comes an intensive study of how the thing is to be made; the choice of materials; the choice of process; the sequence of operations; control and inspection at each operation; provision and flow of materials to converge at the right place at the right time; provision of special jigs and tools; and so on. Further, at various points in the manufacturing process there will be scope for the application of a variety of automatic control devices many of which will be electronically operated.

Technicians are needed for this progress, planning, and automatic control work. These men do not need a thorough scientific training like the technologist. They need some basic science, a sound knowledge of industrial processes, some knowledge of instrumentation, and a certain expertise in applications. They can, at the moment, be adequately trained in part-time courses of the City and Guilds type. The upper limit of the technician group is not sharply defined. It merges gradually into the work of the professional engineer, and many technicians can attain professional status through additional part-time study. The standard of technicians' work is, of course, rising, and before long some kind of sandwich course may be needed to give a more rapid and intensive training.

Supervisors are generally drawn from the ranks of craftsmen or technicians. Their technical training will have been completed before they become supervisors, and what they need is some simple form of management training. As I have said elsewhere, the right approach is for these men to be withdrawn from their employment and given a short full-time course on the elements of industrial administration before taking up their supervisory posts.

Co-operation Between Education and Industry

Co-operation between education and industry is much better than it used to be. Through conferences, discussions, and visits, each has now a better understanding of the problems of the other. This is true both for universities and technical colleges, but there is still scope for improvement. In Britain we have nothing approaching the amount of staff interchange which takes place in America, where people move freely in both directions between industry and teaching. In this respect we are far

more static and I think one reason is the lack of interchangeability in our superannuation schemes. University teachers are on one scheme, technical college teachers on another, and in industry each firm has its own. In these days when hardly anyone can save for retirement the benefits of a superannuation scheme loom large and everyone thinks twice about making a move if he will thereby forgo some superannuation benefit. What we need is some kind of superannuation interchangeability which would permit movement without penalty between the universities, technical colleges and industry.

I have always been a little puzzled by the fact that we have never made much use of the part-time professor from industry whereas the practice is common on the Continent and in America. An example of what I mean would be a Professor of Electrical Machine Design, who was at the same time head of the design department of a large firm. There may be good reasons why we do not follow this practice, but I have never heard any expressed. Is it really quite satisfactory that our engineering graduates may complete a degree course without receiving a single lecture, far less a course of lectures, from anyone not a member of the full-time staff? The position is better in technical colleges, where a fair proportion of the advanced work in part-time courses may be undertaken by visiting lecturers. I am certain we would gain if we had more part-time professorships in our universities and colleges of advanced technology.

A converse problem is the return of teaching staff to industry for 'refresher' training. This should be specially encouraged these days when the rate of industrial development is making it progressively more difficult for everyone to keep up to date. Yet it happens rarely: in the Willis Jackson Report already cited figures are given showing that in 1956 only 34 out of 11,500 technical college teachers returned to industry for periods which might be as little as a few weeks or even days. I am sure industry would do everything it could to help. The fault lies rather with the teachers, who are apparently reluctant to be re-immersed in the turmoil of industry even for short periods.

Co-operative research can be another link between education and industry. The volume of such research is growing but is still considerably less than is found in many American and Continental institutions. In this country we have been very chary, and rightly so, of sponsored research, but in engineering I think our approach has been perhaps too cautious. Engineering research is now so expensive that, without some external help, it is very difficult to break new ground.

In the average technical college research has hitherto made little headway because of heavy teaching time-tables and lack of equipment. The situation is improving, but I should like to see firms going out of their way to invite the co-operation of technical colleges in the solution of some of their production problems. There is not a firm of any size that has not got a whole host of minor production headaches that drag on and are endured because there is no time to sit down and think out solutions. They are not critical problems; they might be described as marginal. Every industrialist knows the kind of thing I mean: the percentage of scrap in some particular operation is unduly high, the layout of a group of machines needs replanning, some particular job is costing too much for lack of a thorough work study, there is a heat-treatment process which will not give consistent results, there is a tricky problem in fine measurement, there is a need for an electronic device to operate an automatic control, and so on. I am sure these problems need not be left unsolved; the colleges would welcome the opportunity to help. It is surprising how different is the British and American attitude to consulting work of this kind. In America industrialists readily consult teaching institutions about the solution of shop problems; here it is relatively rare.

Another service teaching institutions could render industry is to help industrialists to keep up to date. In every science and technology theoretical knowledge

is accumulating at a greater rate than busy industrialists can keep pace with. It is the job of academic people to be abreast of theoretical developments and a liberal provision of short refresher courses should be made available to technical people in industry. It is then up to the technical man to find practical applications for this new knowledge.

My final suggestion in this section is that the engineering industry should establish more research fellowships. There are too few opportunities for the pursuit of research beyond the Ph.D. stage, which, after all, is not much more than a training in research methods. The number of research fellowships in relation to the size of the country's industrial budget is pitifully small.

On these suggestions for more effective co-operation between education and industry I end my address. I said at the start that education was a subject on which it was difficult to say anything new. My address, I fear, bears this out, but the prosperity of the engineering industry is so dependent on sound education and training that periodic reviews of the subject, even if they contribute little that is new, are justified.

ARCHAEOLOGY AS SCIENCE: PURPOSES AND PITFALLS[1]

ADDRESS BY PROFESSOR C. F. C. HAWKES

PRESIDENT OF SECTION H

ARCHAEOLOGISTS often insist today that their subject is a science. In some quarters, they seem to have gained their point: the British Association, anyhow, has for some time now accepted archaeology as fit to share, with anthropology, the title as well as the activities of Section H. Your sectional President this year—and he is not at all unique in this—is qualified for his post (if at all) only as an archaeologist. So perhaps archaeology really is a science. But what sort of a science? The thing has grown up among us, in the last four generations, from two different roots: one in geology and animal and human palaeontology, the other in history and ethnology. As the joint product of biological sciences and what are essentially social studies, therefore, it is often classified as a social science, the fossil branch, so to say, of anthropology and sociology. And for those who dislike the word 'social' there is now the term 'human sciences', which they may perhaps find more cosy. But I want to try, in this Address, to do more than play about with the name of archaeology: I want to ask questions and pass comments about its nature. If, moreover, it is rightly claimed to be a science a species of the social or human genus of the order Sciences—it ought to show itself one not only theoretically, but in practice. Our presence in the British Association should be justified not by our faith alone, but also by our works.

We profess to be able, in the long run, to give an account of the past of mankind throughout a wide domain. This lies between the zones of contact which we have on the one hand with physical anthropology and human biology in general, and on the other with documentary history, and its extensions into the social sciences of modern times, ethnographic and social anthropology. Moreover, we profess to be constructing this account not merely by lighting upon a multitudinous scatter of more or less interesting episodes, but by classifying our discoveries both in space and in time and also in a system of relations, into which there enters the idea of cause and effect, or anyhow of antecedence and consequence. How far we let this last sort of idea govern the relations, depends rather on the point of view we choose: at one extreme, some eyes see a manifold of short-range causations which makes no long-range sense; at the other, some have discerned the operation of general laws. This, of course, is true also of historians. But do we not, just as anthrŏpologists do, think of ourselves as more scientific than historians? Or at least that the immensity of historians' material, and the turbidity caused in it by the frettings and struttings of so many historical human individuals, must prevent their seeing the wood for the trees—I mean, the men, as trees walking? Whereas with us, we tell ourselves, the most scientific and therefore the best, because the purest, kind of archaeology is the prehistoric kind, where individuals are nameless and unknown, and so cannot disturb our studies by throwing any of their proud and angry dust into our eyes.

What then are these studies of ours, about which we can feel so superior, and how does our performance in them justify our feeling so? They are directed towards the enumerating and classifying of all the material remains of human handiwork that can be found, on or under the surface of the earth or under water, into entities or units—industries, cultures, area co-traditions, culture-aggregates, civilisations.

[1] Address delivered at the Dublin Meeting of the British Association on September 5, 1957.

From studying these we can apprehend the kinds of life the people responsible for them lived, where and when and for how long they lived in them, and how well, judged by whatever standards may be applicable, they worked. From cultures and the relations between cultures we can reconstruct the past story of the human race —up to the various points at which we hand it over to the historians or the appropriate anthropologists—in all the aspects we can know and need to know. But of course, since the remains we study are material, we cannot know as many aspects, all by ourselves, as we can know through engaging the help of our colleagues in the natural sciences. We therefore engage as many of these as we can find questions to ask of and can induce to undertake to answer them, and weave the knowledge that they supply into our reports—proudly feeling, as we record our acknowledgements for their services, that their collaboration is not only making our subject more scientific, but also ourselves, surely, more worthy to be accounted scientific archaeologists.

I am of course not scoffing at the collaboration. The constant increase which can be seen today in teamwork between archaeologists and natural scientists is the biggest single factor now operating, at least in the prehistoric field, to improve the quality and extend the range of archaeological research. And if I naturally take a particular pride in the ordinance whereby the holder of my Professorship at Oxford is now *ex officio* secretary of the committee governing the University's new Research Laboratory for Archaeology and the History of Art, that undertaking is only one, and among the youngest, of the many forms of linkage between archaeological and natural-scientific studies which the modern order of things is bringing into being. There is no need, in Section H today, for a Presidential Address on the subject of archaeology *and* science. Haranguing you on that would be preaching in part to the converted, and in ever-growing part to lifelong practising believers. In this Address, I am accepting that very gladly as a fact, which I need not work up to, but can start from. Moreover, once started, I do not intend simply to hold out to you more and more visions of the archaeological triumphs that are bound to come from more and more collaboration with the natural sciences. My theme is not archaeology *and* science, but archaeology *as* science. I believe we ought to be thinking more about our purposes, when we call our subject so splendiferously science; and to be asking whether there are not pitfalls along the road ahead, which we must avoid if we are to continue, as I hope we shall, to keep the splendour bright.

I said just now that archaeology has one of its two roots in geology and palaeontology, that is, in evolutionary natural history. If we start our studies at this end, we put ourselves into the closest possible contact with geology, and with palaeontology, both that of man himself, and of the contemporary fauna, and also that of the flora, which is palaeobotany. We are studying man in his place in the biome, and we watch him gradually achieving more control in it, and a social life better fitted for the continuance of this process. The process is not, as used to be imagined, unilinear: it quickly becomes multilinear, and our purpose is to follow all the lines, mark their interactions, and discern on which of them, at what points, occur the more decisive steps, the major inventions and discoveries—animal domestication, agriculture, pottery, metallurgy, the wheel, the sailing-ship, and the rest of them. And with technology and the economic activities based on it, we look also for the social developments, in their variety and their interaction, and for the religious institutions that will accompany and reflect those of material existence. This may be called, after the well-known small book by Gordon Childe, the Man Makes Himself school of thought; and the articulation of archaeology and the appropriate natural sciences which gives effect to it in action, beginning with earliest known human manifestations and working forwards from them, is what my colleague Grahame Clark, one of its altogether best exponents, has called primary prehistory.

Now primary prehistory is indispensable. Quite obviously, there could be no

full-length story of mankind without it. And its purpose is unexceptionable: to cover the whole great tract of that story, emphasising man's place in the biome of which he is a part, in every branch of the story and at every stage, from the beginnings up to where historical considerations start to supervene. Moreover, it can be presented as a unitary matter. The study of the Palaeolithic is essentially world-wide, and so is that of the Mesolithic following it. As the science of man in the vast stretches of Pleistocene and Holocene time in which he has lived by gathering, hunting, and fishing, and had no use of those specific techniques which have brought the self-reliant economic and social life of civilisation into being, the primary prehistory that we possess today, and are watching continually advancing to greater mastery of its subject-matter, has an essential place among the intellectual possessions which make our modern outlook upon the world different from any previously attained. And furthermore, this same prehistory will carry us through the transition from hunter-fisher life to life in which communities grew vegetable or animal food of their own, and formed stable social groups conditioned by one or another mode of this new kind of subsistence, which is the essential of the Neolithic. That transition in the Old World—for America of course has it in a version or versions peculiar to itself, requiring a study and even nomenclature of its own—took place in the Near or Middle East, and is being progressively studied there today. In the various Asiatic regions to which the new kind of life was spread, its adoption and variation can be studied likewise, as it can in Africa beginning in or close to Egypt, and in Europe beginning with the new German discovery in northern Greece, that Neolithic farmers who as yet possessed no pottery, and in this respect resembles those demonstrated for the Near East first by Dr. Kathleen Kenyon at Jericho, settled also upon the plain of Thessaly.

Thenceforward each region can offer the archaeologist a study of the incoming, in one manner or another, of the new subsistence, and the action and reaction ensuing between the communities who brought it, and the hitherto essentially hunter-fisher natives. In Europe, this is primarily what archaeology is studying for a period in the south-east doubtless well before 3000 B.C., and continuing, in all save the parts in closest connection with the Near Eastern civilisations that had grown up meanwhile, until after 2000. By then, of course, knowledge had come in not only of pottery-making, which has been found present in all European Neolithic cultures excepting the most primitive, but also of metallurgy, first in the form of copper-working. We are approaching the study of our European Bronze Age; and our Iron Age is then only ten to fifteen centuries ahead.

How scientific, then, has all this primary prehistory been? To reason from observed facts by inference, as it does, and from general principles by deduction, as if there are any it must, is of course not a property of scientific method only—unless all thought is science which conforms to elementary logic. Its distinctively scientific characters seem to be, that it excavates its sites according to the principles of stratigraphy and association which it has borrowed from geology, that it makes exact measurements wherever necessary, and the exactest possible comparisons when it has to classify its material, and that it hands over everything to a natural scientist of which identification or analysis by him will have anything to reveal that may aid archaeological interpretation. Let me enlarge briefly on this last matter first. You can hope for determinations of absolute age, from the late Pleistocene onwards, by the Carbon 14 method, which will pin down the latter end (or ought to) of Milanković's curve of solar radiation, and at the same time supply the needful check on the late- and post-glacial varve chronology of De Geer; for determinations of climatic age throughout the Pleistocene and early Holocene by geology and palaeontology, and from the late Pleistocene onwards again by palaeobotany, to which the Carbon 14 method just mentioned can supply absolute figures of age in years; for determinations of their appropriate specimens by the zoologist,

malacologist, plant-botanist, soil-scientist, and petrologist; for reports on human remains by the physical anthropologist; for analyses of metallic and vitreous materials by optical or by X-ray spectrometry, as well as by the older laboratory methods; for age-determinations of suitably-preserved ceramic material by measurement of thermo-remanent magnetism; for whatever information about any kind of ceramic material can be supplied by heavy-mineral analysis or by the new method of neutron-activation; and for all that the modern museum laboratory can tell you about the composition and mode of manufacture of anything and everything you may send to it to be examined and restored. Of course, not primary prehistorians only, but all kinds of archaeologist, can hope for some at least, if not all, of this wealth of potential scientific information. But, whatever kind of archaeologist or prehistorian you are, your claim to scientific status here, for yourself, rests only on the care which you take to provide as far as possible properly guaranteed, undamaged, and uncontaminated specimens for the appropriate examination. And that this is a genuine claim, and not simply a matter of taking commonsense precautions against getting things mixed up, broken up, or messed up, must depend on the principles embodied in your part, our part, the archaeologist's part, of the interpretation of the find. If these are scientific, then we to that extent are scientific; if they are not, we are here no more than painstaking and inquisitive individuals who have managed to get some information from other people who are scientists while we are not.

What are the archaeologist's principles, which are embodied in his interpretation of his excavations, measurements, and comparisons? I have often asked myself this question, and I really believe that there are only two. The first is simply the tenet, that people in the past did things for purposes which can, in most cases if not in all, be correctly understood. The second is, that if cases are found where different people did the same or closely similar things, when not separated in time or space or circumstance to a degree which appears to exclude all significant connexion between them, then their doing these same or similar things is not fortuitous, but requires a single—whether simple or complex—explanation to cover them together. The easiest form of this is where the cases are all within the same measurable limits of time and space, and where the things done are really the same or very closely similar, and are numerous. Where this is so, and according to the degrees in which it is so, we relate all the things together, and say that we have found an industry, a culture, an area co-tradition, or whatever entity or unit is appropriate: the ultimate ones are culture-aggregates or civilisations. Where time and space are variously extended, and the things similar are less numerous or weighty than things in the picture which we see are different, we say we have found this or that evidence of connexion between two or more of our cultures or other entities or units, which differ otherwise. That is to say, we explain like by like, wherever possible. And this principle rests on the observed facts of human conservatism: within any culture, people will not easily make a sharp break with what their fathers and mothers have done, so that there will never be novelties without good reasons, and reasons when we adduce them should be as few as possible—like essences in the scholastic philosophy of Occam's Razor, 'not to be multiplied beyond necessity'.

What other principles of archaeological interpretation there are, besides these and the general rules of excavating stratigraphically, measuring accurately, identifying by comparison, and collaborating with experts of other disciplines wherever useful, I do not know. For primary prehistory, at any rate, they seem to me to be all-sufficient, since it does not have to take account of specifically historical considerations drawn from written texts. And even when we leave this 'text-free' domain, and venture into the various archaeologies which are to any extent 'text-aided', since they do have to reckon with historical considerations of that kind—Early Christian archaeology, or the archaeology of the Roman Empire, or the Greek

Colonies, or the European Colonies in America—the principles are only extended in one respect. But it is an important one. It is that the archaeologist must either read the texts, and know what the relevant historical considerations are, or else have the collaboration of historians, philologists, or the like, who can read and know them and will guide him. Some people, indeed, seem to feel that this sort of archaeology is not the best because not the purest sort. Yet this extension of our principles really introduces nothing but a fresh aspect of the principle we have embraced already, of consulting experts whenever it will be useful, which requires our liaisons with natural scientists. How useful it will be of course depends on the experts' reliability; but that applies both to humanistic experts and to natural-scientific ones. For both likewise, some questions are easy to answer reliably, some less easy, and some impossible. Also, the different experts give us information of different sorts. Broadly speaking, the information we get from natural scientists concerns the composition, origin, and antiquity of materials; what we get from humanistic experts is information of other kinds; and each may affect our archaeological interpretations in ways in which the other cannot. None the less, the principle of collaboration is the same for both. Thus, in my submission, the principles on which archaeology as such must rest are no more, in whatever circumstances, than these simple few.

It must be by observing these simple few principles, then, that we become entitled to the epithet 'scientific'. Is this too easy? Surely not? Surely we are all scientists now? Of course, observing the principles means not just believing in them theoretically, but putting them into practice, and doing everything to facilitate their observance by our archaeological community in general. That seems obvious; and in this way, all our purposes will in the end be realised. As for pitfalls, we could only fall into them by neglecting the principles, so it is pretty clear that any there may be will be safely circumvented. Let me then lead you on a brief tour of inspection of our affairs. It ought to be encouraging; and it should give archaeology a good advertisement in British Association circles.

Beginning with the Palaeolithic, even though it happens to be absent, apparently, from Ireland, we can survey our general progress on it with satisfaction. If there is still controversy over glacial correlations, and in the hypsimetric and bathymetric study of terraces and shorelines, those are mainly the business of the geologists; we need not doubt that they will get most things right in time; and meanwhile, our stone industries are being re-studied by statistical methods, and while Upper Palaeolithic origins admittedly are still rather a problem, we can see much better than formerly where the problems lie, and how we should direct the technological analyses whence must come our main contributions to the solving of them. We know about the economics of the hunting life which the Upper Palaeolithic evidence especially well reveals; we can estimate the size of the social group in terms of hunting strength and of winter house-size; and we realise that there will have been considerable seasonal movement following the game. Lastly, we can interpret Upper Palaeolithic art in terms of hunting-magic, and can see in it the germs of a religion of animal fertility, giving big herds to hunt, upon the one hand, and of human fertility, on the other, reflected in the Venus statuettes. The L'Angles-sur-l'Anglin figurations must indeed carry our thoughts still further, and we recognise that there is much scratching and dabbing of conventional signs on Upper Palaeolithic cave walls, in addition to the animal and rare human portrayals, which we shall never understand. But when we find the dead buried respectfully, and sometimes with ornaments and supplies, we can infer that the life they were thought to have gone on to was imagined in terms of that lived by the living, as our researches have revealed it. Taken all round, then, Palaeolithic life, right up to the highly-organised hunting life of the best-known Upper Palaeolithic peoples, is a thing of which archaeology, together with the other sciences contributing to primary prehistory,

can give us a sufficient picture. For the chronology of its later phases, too, we have precious information beginning to come to us from Carbon 14 determinations.

Of the Mesolithic, though with a less immediate impressiveness, the same is broadly true. The technology of equipment for hunting, fishing, and working stone and all animal and plant products that were usable, can be studied in considerable detail; the economic life, a hunting and fishing life essentially, can be readily reconstructed once sufficient work, in collaboration with natural scientists, has been devoted to the manifold evidences which can be got for it; the social and religious life stands to be interpreted only in these same terms; and the evidence of art, when we can get it, tells the same tale—although conventionalising of forms is carried further now, and there is still a remainder of unintelligible signs.

Lastly, when we go forward to the Neolithic, whether we study its inception in the Near or Middle East, or the diffusion of this new mode of subsistence by farming outwards thence to new lands and coasts such as those of Europe, the thing can still be understood by the primary prehistorian's methods: study of the technology and material remains, study of the environment, inferring thence to the economic life, and as far as possible the social life likewise, and use of the art (where it is more than merely decorative or skeuomorphic), along with what may be learnt from the disposal of the dead, to outline the cultures' spiritual or religious content, which will appear as a reflection from their economic and social content. The reflection is indeed more coloured now by symbolism, in that the fertility-religion of farming communities is directed at fertility not only human and animal, but also vegetable, that is, the fertility of the earth that makes the crops grow. This last can only be portrayed symbolically, and does appear to be so portrayed, in human female form. Male imagery is less frequent, or less obvious, but is present none the less; and once all such symbolism is accepted as covering fertility in all the forms in which Neolithic farmers must obviously have desired it, the interpretation of the spiritual side of these cultures from the material, by the same methods of primary prehistory, can be as thorough for them as for those of hunter-fishers. Movement across the map, which in the Palaeolithic and Mesolithic can be explained almost entirely by climatic factors, can in the Neolithic be understood more directly as a search for land suited to the farmer, reinforced by gradual increase of population. And in view of the map's diversity, a variety of waves, groups, and regional differentiations is to be expected amongst Neolithic cultures. Their area co-traditions will become diversified; and we have also to mark the rise of secondary Neolithic cultures among previous hunter-fishers on whom influences from the primary Neolithic, directly or indirectly, acted.

All this is intelligible to reasoning based wholly on material and environmental factors; and were there no more to the thing than that, we could expect to continue satisfied with the same mode of reasoning right through the diffusion of metallurgy which followed, issuing from the old Near-Eastern centres which by this time are becoming gradually more and more historical, and so on through the Bronze and even through the Iron Age. Everybody knows that there is more, and more in increasing measure as time goes on, to Bronze and Iron Age archaeology than that. But prehistorians, I think, have usually felt able to postpone, to dates quite comfortably late, the time when they have to start admitting that their prehistory is no longer primary. We in Europe, thus, can watch the Orient becoming more and more historical through the third and the second millennia B.C., and the Aegean showing civilisation almost as high in Middle and Late Minoan and Mycenean times, without seeing in these anything more significant, for our contemporary but still prehistoric continent and islands, than a source of convenient pin-points in our chronology such as we have made for the second millennium with amber necklace-spacers or segmented faience beads, or, going forward to the first, in the Greek exports of the Heuneburg or Vix.

Such indications of date, and of long-distant contacts, are indeed most precious, as the excavations at Tara, in the former case, have lately shown in Ireland. But this review of our affairs has here brought me to a point when I have to pause in it, and declare to you my conviction that European prehistory today, in the second as well as in the first millennium B.C., can less and less justifiably be treated as altogether primary. And the reasons for this come in the first place from phenomena which we have to recognise as complicating our primary prehistoric scene, whether we regard the Aegean and Near East or not. How can the phenomena of Bell-beakers and their associations in Europe be accounted for by primary prehistorian's reasoning unassisted? There is nothing demonstrably climatic, nor arguable from the mere idea of food-quest, to explain their diffusion over so much of Europe in the early second millennium: we need to know more before we can account for them. Similarly, what explains the sudden appearance of Globular-Amphora pottery, which has its nearest counterparts on the South Russian steppe, in the Polish and North and Middle German plain? And what explains the ensuing proliferation and diffusion, apparently from that same quarter, of people burying their dead under round barrows, singly, and with stone shaft-hole axes and pottery embellished with cord-impressions? And how did this and the Bell-beaker movement manage to get mixed up, as the Rhineland and these islands show they did? Inevitably, when confronted with this kind of thing, one's mind has to pass beyond the primary level of reasoning; not to let it do so would be less than scientific, because failing to face the evidence. Much indeed does stand to be accounted for on that level; but if one stays wholly on it one will miss the rest. One must admit that, surely; and when one goes forward in time, the situation gets no easier. I am not complaining: I am only trying to be honest; and if what I have just said in the first place may sound negative, it is not all.

In the second place, we archaeologists in Europe have to face the fact that in that same first half of the second millennium languages were being spoken, in Anatolia and also in Greece, a Hittite language and an early form of Greek, which belong to the Indo-European linguistic family. In Greece, Greek has been spoken ever since; and over the greater remaining part of Europe (to say nothing of parts of Asia), evidence from the first millennium B.C. or early A.D. shows languages of the same family variously spread, the most successful of them being ancestral to nearly all modern European tongues. Moreover, when that early Greek is revealed to us in the Linear B Minoan and Mycenean tablets, it gives evidence which can be used, in conjunction with later Greek evidence and with reasoning on comparative-philological principles, to augment considerably the conclusions which philologists had already drawn about the antiquity of social and religious institutions common to all the speakers of languages of that Indo-European family. Some may say that we do not need to face these facts. I say, that the time has now come when we must begin to.

For the last thirty years it has been usual amongst us prehistorians—all except those whom extreme learning or extreme ignorance has made the bravest—to ignore comparative philology. This is partly because we have been told that if we tried to meddle in it, or in such a part of it as the Celtic part, we should fail to understand it properly: we have not wanted to make idiots of ourselves. It is partly also, because we have felt able to plead either of two alternative defences for not caring at all about the subject, and in particular not for Indo-European comparative philology in connexion with European prehistoric archaeology. Either we plead that we are bound to study prehistory in its primary mode exclusively, that is, with the help of materially-based natural sciences only, so that philology is irrelevant to it and to us; or, alternatively, that Indo-European languages can be believed diffused quite far enough over Europe around the beginning of the second millennium already, namely, by the single-grave/shaft-hole axe/corded-pottery peoples that I have just

now mentioned, for us to be justified in thinking as little as possible about them from that time onward. Of the second of these defences I will only say, that as only a couple of scraps of pottery embellished with any sort of cord-impression have ever been found in Greece, and none at all in Anatolia, and as the whole Aegean-Anatolian region had perfectly good shaft-hole axes before the beginning of the second millennium altogether, the idea of making this expansion responsible for carrying Greeks and Hittities there, which could only ever be sustained by special pleading, has now among all serious students of the matter had to be abandoned altogether. Thus, the idea of making it responsible for carrying other Indo-European languages to their historic homes, as the Teutonic ones to Northern Europe, or the Celtic ones to Western or the Italic ones to Italy, starts, to say the the least, with a heavy handicap; for Greek and this Hittite language are the only two such known as early as this time at all. As for the first defence, we have already seen that the truly-scientific archaeologist will seek collaboration not with natural-scientific colleagues only, but also with humanistic ones where it will be useful. Among these latter are comparative philologists. Our ignoring of them during the past generation has been not, I think, just because they are philologists and we bar that kind of cad on principle, but because we have thought their collaboration not useful; and why not? Because of a conviction that they were not to be trusted. For those of us interested primarily in British Celtic, 'they' usually meant (whether or not we knew it) just Sir John Rhŷs; but how far the conviction was wrong and how far right matters not much today, for today is not 1927 but 1957.

The conviction that I want to suggest we adopt in 1957 is, that wherever people of cultures being studied archaeologically come anywhere near the purview of evidence bearing on their language, or on institutions known from such language, we should have some few among us anyhow who will set about asking what that evidence is, where it can be found assessed, and how far it can be trusted. And those few should not be dismissed as cranks, who want to distort good archaeological findings to make them fit philologists' airy theories: when they have something to say, they should be listened to. And with that, I will suggest that a second conviction be adopted among us, parallel with the first. It is that the philologists too, on their side, should have anyhow some few among them ready to be asking what we, the archaeologists, have that we should be telling them about our findings, and how far these are airy theories, and how far to be trusted. What we can tell them certainly extends more widely and farther back than much of what they can tell us. But it does not stretch farther than does general linguistic theory; while as for the basic groupings of known human language, their beginnings must infallibly, I should say, be anyhow of Upper Palaeolithic age. And for our Old World and Near Eastern and European fields, as the Linear B discoveries have shown, and for our especial field in these islands, as recent work both in Irish spheres and in Professor Kenneth Jackson's British and Pictish spheres have shown, there is a great deal in philology today which there was not thirty years ago, which we ought to be getting to know about. Are we afraid of it? What we ought to be is not afraid but confident: confident that our great and growing archaeological knowledge of the past has much in it that the philologist should share, and that the better we get to know him, the more we may be able to learn of his knowledge that we should share. And I say of this attitude, of eager confidence, compared with our like attitude towards the natural scientist, that each, if properly conducted, is as scientific as the other. If, as I think may well be, that is better understood in some Irish circles than it is in some in Britain, it is yet one more reason for being thankful that we are here for this meeting, and for marking what Irish scholars will be telling us about its Irish aspect before the meeting ends. And if—to go back to my earlier example—Indo-European languages and institutions were not spread in Europe by makers of cord-ornamented pottery, we ought to want to know how they were spread. For by far the longest part of his

existence as a species, man has lived in immediate dependence upon nature, and to the unique value of the illumination of those long stages by prehistory in the primary mode, I have already testified. But the more mankind has transcended that dependence, and the more his cultures show us which is not directly explicable by reference to his economic basis of subsistence, the more should we likewise transcend prehistory in the primary mode, wherever we may be lucky enough to find other lights, in more varied colours, for turning on.

Our purpose ought to be to welcome everything, no matter whether from natural-scientific or from humanistic sources provided we can trust it, which will illumine not prehistory in the pure sense only, but the transitions from prehistory to history as well. Our job deserves glorifying in all its parts. The pitfall is to imagine that humanistic liaisons contaminate our scientific character. For what is this character? Do not let us fancy that our natural-scientist friends like and admire us because they regard us as scientists just like themselves. It is because we are not that, but on the contrary are students of human things, whose work in some ways interlocks with theirs and requires a mental attitude harmonious with theirs, even though our concern is in fact not electricity but society, that they are in various degrees prepared to find us interesting. It is true that some of our more traditionally humanistic colleagues do not find us as interesting as we might like. But time is on our side there, and I think their interest can be won.

Moreover, we must not forget our colleagues in ethnographic and social anthropology. Since it ceased to be believed that modern primitive societies are specifically, instead of quite generally only, 'representatives' of our prehistoric ones, we prehistorians are not felt (otherwise than in the American field, for example, which is unusually susceptible of being studied as a whole meaningful in all its parts, ancient and modern) to have any particularly significant message for our colleagues in those realms of anthropological science. Having only material remains to study—and in spite of such light on ancient ways, in Europe, for instance, as can be thrown by folk-life and folk-lore studies, as will be illustrated, chiefly for Ireland, at this meeting—we are held to be unable to apprehend our prehistoric cultures as functional wholes. Yet, as I have shown, the primary prehistorian does now in some measure do this. And as for the transitions from his fields of study to those of historical times, the more we study also these, the less we lie open to that charge. We shall be linking up primary prehistory ever more with the apprehension of functional wholeness that becomes possible with the aid of history and the other documentary human sciences—such as anthropology itself, as more and more of its field studies are by time turned into classics, must more and more become. At present, the trouble about non-archaeological anthropology, when history is closed to it, is that it cannot by itself be adequately diachronic; the trouble about archaeology, that it can by itself reveal too little that is non-material. But by spanning as far as we find possible the gulf between primary prehistory and the documentary human sciences, we can reinforce archaeology's claim to its traditional place in the functional whole of anthropological society.

I have said enough about our faith. In conclusion now, I turn to our works, by which also, as I said earlier, we must be justified. Methodical, stratigraphic excavation—but does this, amongst us, really need a discourse? Accurate measurements —do we need to be warned against not taking them? Comparisons? Well, obviously, interpretation of an excavated site, measurements and all, will be helped by that of any other that has previously been excavated like it. Without excavation, likewise, comparison is what enables the classification of the antiquities of the Irish or any other countryside. But we all know how disappointing it is when there are no finds. A findless excavation, a stretch of countryside that has never produced any chance finds, that will contribute nothing to a distribution-map—if we had too much of that sort of thing to deal with, we could make far too little out of archaeology.

Without finds, and our immensely proliferated activity of comparison, leading to their classification in the systems on which we make everything stand up, we should soon be unemployed. So I am going to take from this, this business of dealing with our finds, my concluding theme. And as it is concerned with works, what I say on it will be practical.

Our claim to have archaeology reckoned a science, even if a human one, and on the strength of it to be justified in seeking the collaboration of natural scientists and humanistic scholars when we need them, is bound up with our ability to compare and classify our finds, and to construct systems on the classifications so obtained. It must be our purpose perpetually to be prosecuting and refining all this business. Pitfalls can only threaten us if we neglect it. We all believe and know this. But what is happening in practice? Forty and even thirty years ago, when many Victorian-born writers' books could still be new, the simple classifications they had made still met one's needs. One could probably find all one wanted, for comparison or identification of new material, in them—from Wilde and Anderson and Evans, through Mortimer, Coffey, Abercromby, Reginald Smith and Armstrong, to the brash young Fox or Wheeler. Periodical literature, too, was not too bulky to be kept up with as one went along. Today, not only is our periodical literature vast in its extent, but it has become almost the only kind of literature that is written to be fundamental. Books, with very rare exceptions of which I need only cite Piggott's on our Neolithic cultures, are all introductory or popular or semi-popular affairs. Meanwhile, the Council for British Archaeology has to go to great pains to publish a bibliography of new work each two years. And the more serious trouble is that it is becoming harder and harder for writers of new work, excavators writing their reports, or whoever it may be publishing some new find, to find the published comparisons they need, let alone what is extant but unpublished. The result is, that without the facilities of the very few really big libraries, and a ruthlessness in plaguing museum officials which would destroy its own object if everyone practised it at once, one can all too seldom properly do one's stuff; while, if one has access to those libraries and knows them, and is either so loved or so feared by one's museum friends that they will answer all one's questions, the task of doing one's stuff is apt to be so lengthy and laborious, that far too little of it will get done at all.

I am all for the rigour of the game. But this situation is not the glorious austerity of science; it is just sheer muddle. Also, it is making the attractive face that archaeology turns to the outside public, through broadcasting, television, journalism and little books, increasingly a sham. We keep telling the public how wonderful we are, and how efficient, when if they only knew more about us, they would find us becoming steadily worse hampered by failure to organise our raw material. Is this scientific? The natural sciences indeed have corresponding problems, but at least they put their basic information into works of reference. And so with scholarship. All classical texts can be consulted. Inscriptions are collected into corpuses. So indeed, too, with classical archaeology, with its vases, coins, and sculpture. Museums, too, the repositories of most archaeological material, have been expected to issue catalogues. In Dublin, this year 1957 marks the centenary of the publication of Sir William Wilde's *Catalogue of the Museum of the Royal Irish Academy*. But how many new museum catalogues are coming out this year, or will in five or ten years, of archaeological material from these islands? Very few. It is too expensive, and the staffs will always complain that they are too hard-worked already.

And really, we need more than individual museum catalogues. What we need, for all our antiquities on uniform lines, is a corpus treatment. Not in books, far too cumbersome, but on cards. All who subscribed could obtain photographic copies, either of all of the master set, or of parts of it as desired. The undertaking called *Inventaria Archaeologica*, which uses printed cards, has started to cover important associated group-finds; but that is a selection only. It ought to be backed

up by card-corpuses, reproduced by a less expensive photographic method, for all the chief bodies of our entire material. I suppose the drawing, by two or more skilled draughtsmen going gradually round, could be done in five years for £10,000. Meanwhile, a federation of central institutions, in our various capital cities, would have to start keeping the thing annually up to date. The classifications would have to be simple, to stand up to the advance of knowledge: this is a corpus, not a gigantic text-book. But, by these means, the material facts of our archaeology would be placed within reach of all who wanted the advance of knowledge, and not, as at present confined to the doubtful capacity of a few frail so-called expert heads. Cannot this be done? Then, indeed, we could claim, and not by faith only but by works too, our archaeology as science.

CHEMICAL TRANSMISSION AT NERVE ENDINGS[1]

ADDRESS BY PROF. SIR LINDOR BROWN, C.B.E., Sec.R.S.

PRESIDENT OF SECTION I

THE general theme of the first two days of this Section is the mode of action of the chemical transmitters of the effects of nerves. I intend my remarks to serve as an introduction to some more specialised papers; I hope to indicate in a general way the nature of the problems encountered and why those who deal with them find them of such absorbing interest.

First I should define some terms and impose some limits on a subject which without them might embrace much of physiology. It is convenient for everyday use to distinguish between conduction and transmission; if the term conduction is confined to events occurring in one cell or a syncytium, and transmission to communication between cells, confusion is less likely to arise. One may then refer to conduction of the nervous impulse, to conduction of the excitation wave along a muscle or throughout the heart, and on the other hand to synaptic transmission between the processes of a nerve cell and the processes or body of another, and to neuro-effector transmission between a nerve and a muscle cell or a gland cell. It is to this last, neuro-effector transmission, that I intend to limit my remarks, although an occasional reference to synaptic transmission will probably be unavoidable.

The effector cells are those through which the body reacts to changes in its environment; they produce the end-effects of reflex and voluntary action. In mass and importance the striped muscle of the skeleton is the predominating effector organ of the body because, in the long run, it is only through muscular movement that the human brain can express itself. Speech, writing, the graphic arts and music depend in the final analysis upon muscular movement. But muscular movement for its maintenance and effectiveness depends upon adjustments in blood supply, and every contraction of skeletal muscle involves reflex adjustments in blood-flow and heart-rate; it involves in other words two other muscular effectors, the smooth muscle of the small arteries and the specialised muscle of the heart. The other hollow organs of the body, the urinary and reproductive systems supply a mass of smooth muscle which is the effector organ of many reflexes, and the great tube of the alimentary canal provides effectors not only in its muscular walls, but in the gland cells upon whose delicate control depends so much of our inner comfort. We see then that the skeletal muscle effectors are supported by a host of effectors in the smooth muscle and glands of the body, less obtrusive in their action, but none the less important and sometimes even serving to influence the environment, as those who have been affected by a blush or a tear can bear witness.

It is how impulses conducted in nerves from the central nervous system influence the activity of the tissues upon which they impinge that is my primary concern. The nerve cell and its long process, the axone, must be considered as one closed system in which, at rest, the inside is held at a potential of 50–90 mV negative to the outside. This impinges upon another closed system, the cell of the effector. This structure has the property of being excitable and of conducting impulses or waves of self-propagating activity. In all excitable cells, the conducted impulse is

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brought about by a sudden change in the properties of the bounding membrane which permits the flow of electric currents and excites the same change in neighbouring parts of the membrane. The process by which a cell influences another—the process of transmission—involves, as we know, something more than this electrical spread; specific substances are discharged at the nerve ending which excite or depress the next cell in the chain.

In the case of skeletal muscle, the anatomical pathway through which the nervous system influences its effector is simple and direct; the nerve fibre leaves the ventral horns cell of the spinal cord and proceeds directly to the muscle which it supplies, there to divide and terminate at the motor end-plates of a group of single muscle fibres, the ratio between nerve and muscle fibres being anything up to 1:300. This is the somatic or voluntary system. The effectors in hollow organs and glands are supplied by nerves of the involuntary nervous system which follow a more complex route, in that their path is invariably interrupted by intermediate nerve cells located in a ganglion. Just as the motor nerve to a muscle divides and supplies many muscle cells, so the preganglionic fibre divides and supplies many ganglion cells; one result of this is that activity in many parts of the involuntary nervous system cannot be precisely controlled from the centre. In one division of the involuntary nervous system, the craniosacral outflow or parasympathetic, the ganglia are near or actually within the organ supplied, as in the supply to the heart and the stomach from the vagus nerve. In the stomach and intestine the ratio between preganglionic vagal fibres and ganglion cells must reach astronomical proportions since the wall of the whole gastro-intestinal tract is beset with ganglion cells and yet the number of efferent fibres of the vagus nerve passing through the diaphragm is probably less than three thousand.

In all this complexity, however, there appears one broad generalisation; it is now established beyond reasonable doubt that transmission from both nerve to skeletal muscle and from preganglionic fibre to ganglion cell is by the liberation of acetylcholine; in other words, all the efferent fibres from the central nervous system are cholinergic. Our knowledge of the events occurring at the neuro-skeletal muscular junction is now extensive and requires from me no more than the briefest summary. At a single neuromuscular junction, there is a large number of units, probably considerably exceeding a hundred in number, capable of liberating a small charge of acetylcholine from a preformed store. This liberation, in fact, takes place in a random fashion continuously, but the amount from a single unit is much too small to excite and cause contraction of the muscle. With the arrival of the nerve impulse a hundred or more of the units discharge their acetylcholine simultaneously, and it reacts with the receptor in the muscle fibre. This reaction leads to a short-circuiting of the end-plate membrane, a flow of current and excitation of the muscle fibre; the excitation wave then spreads along muscle and contraction follows. It is possible to record these events electrically, the most elegant method being to impale the region of the muscle end-plate with a micro-pipette some 0·5 μ or less in diameter. Records so taken show that the electrical change at the amphibian motor end-plate rises to a peak in about 1 msec, decays to one half in 2 msec and then declines more slowly. The reaction between transmitter and receptor takes place at the early stages only, lasting little longer than the rising phase. This curtailment of transmitter action is due to the destruction or inactivation of the transmitter by the enzyme cholinesterase which is known to be concentrated at the end-plate. One author goes so far as to suggest that the receptor on the muscle fibre is the enzyme itself.

Much of our knowledge of neuro-skeletal muscular transmission is due to two facts; first, that quite early in the history of physiology the arrow poison curare was found to have the specific and peculiar effect of blocking neuromuscular transmission, and secondly that the enzyme cholinesterase can be inhibited by

eserine. Curare produces its block by competing with the liberated acetylcholine for the end-plate receptors. It has been an invaluable tool in disentangling the electrical events occurring at the end-plate. Eserine, by protecting the liberated acetylcholine from destruction, prolongs and intensifies transmitter action, allows the accumulation of the acetylcholine and has permitted its collection and estimation in the fluids surrounding a muscle.

We have then a clear and detailed picture of the transmission process at the neuro-skeletal muscular junction: the arriving nerve impulse evokes a momentary synchronisation of the discharge of the acetylcholine units at the nerve endings, the liberated acetylcholine reacts with the receptors, and in about 1 msec is inactivated by cholinesterase. Its action is in fact precise, localised and transient; it does not persist at its site of liberation, and it does not diffuse beyond the very narrow limits of its site of action.

At the ganglionic junction transmission from preganglionic fibre to ganglion cell follows a similar course, although our knowledge of the details of the process is much scantier than in muscle—largely because of the great difficulty there is in impaling ganglion cells with microelectrodes. Transmitter action is much more prolonged than in muscle and the anticholinesterases have much less obvious effect. It may reasonably be assumed that the disappearance of the transmitter is due more to diffusion from its site of action than to its inactivation by cholinesterase.

We have dealt now with the transmission of impulses at the ends of the efferent nerves from the central nervous system. You will remember that in the involuntary nervous system supplying the heart, the blood vessels and the smooth muscle and glands of the hollow organs the efferent nerves are relayed at the ganglion, and the *post*-ganglionic fibres make contact with the effector organ. These post-ganglionic fibres transmit to their effectors either by the medium of acetylcholine or by liberating another substance, adrenaline or the closely allied noradrenaline. In other words, the postganglionic nerves may be either cholinergic or adrenergic. These postganglionic fibres control tissues which have extremely diverse structures and functions, and I must attempt to produce some broad simplifications to prevent the argument becoming too complex. Let us say that in general the cholinergic nerves depress the action of the heart and cause relaxation of the smooth muscle surrounding the small arteries. Adrenergic nerves on the other hand accelerate and increase the beat of the heart and cause contraction of the muscle of the arteries.

Later in this meeting you will hear a detailed account of how the vagus nerve acts upon the muscle of the heart. We know little indeed about the processes of liberation of acetylcholine from the nerve endings except that it is a much slower and more deliberate affair than in skeletal muscle or ganglion. The decline of transmitter action can be prolonged by giving eserine; so in the heart again, the destruction of acetylcholine by cholinesterase is an important factor in limiting the action of the vagus nerve.

Although at first sight the action of liberated acetylcholine on skeletal muscle which it excites and on the heart whose rhythmic activity it depresses would seem likely to be two entirely different processes, recent work suggests that in both cases the fundamental change is an alteration in the permeability to ions of the membrane bounding the muscle. At the motor nerve ending acetylcholine produces a sudden non-specific breakdown in the membrane resistance so that any ion can pass it and carry current. In the heart, the change appears to be an increase in the permeability of the membrane to potassium and possibly to chloride ions only. This has the effect of increasing the polarisation of the muscle membrane and reducing its tendency to beat spontaneously. It might be expected that the adrenergic nerves whose effects are opposite to those of the cholinergic nerves might act by decreasing

the permeability to potassium ions, but there is no experimental evidence for this. The mode of action of adrenaline on the heart remains for the moment obscure.

Although some light has been thrown on the action of the transmitters on the heart, we know practically nothing about their effects on the smooth muscle of other parts of the circulatory system. A similar obscurity veils their action on the other smooth muscle of the body. The reason for this is the great difficulty of recording from the effector organs because smooth muscle cells are extremely minute, they show spontaneous changes in activity, and their very isolation into single units is a matter of doubt. In tissues like the intestine, acetylcholine is an excitant and, as one might expect, tends to cause a depolarisation of the muscle membrane; adrenaline, on the other hand, appears to cause a hyperpolarisation and acts as a depressant of muscular activity. Gland cells supplied by cholinergic and adrenergic nerves have also been investigated. In the case of the salivary glands both nerves can cause activity and secretion. The ionic movements into and out of the cell so produced are complicated by the movement out of the cell of its specific secretion. The study of the innervation of secretory cells by the conventional electrophysiological methods is in its infancy. The eventual solution of the mode of action of the nerves controlling secretion is going to be a fascinating story, but it will be a long time before it is told in detail.

We have now considered the bare bones of the modes of action on effector cells of the three known chemical transmitters, acetylcholine, adrenaline and noradrenaline. I should like to turn now for a moment to think about the liberation of the transmitters and their removal from their site of action. Although the nerve impulse is an 'All-or-Nothing' phenomenon, there can be no doubt that the amount of transmitter liberated at the terminals is variable. At the neuroskeletal muscle junction, previous activity of the ending can greatly increase the number of units contributing to the liberated charge of transmitter, and this phenomenon is probably common to all junctions; it is particularly evident at the junctions in the central nervous system. Continuous, unremitting activity can, on the other hand, produce a falling off of the amount of transmitter liberated by each nerve impulse, and in isolated amphibian muscle it can occur to such an extent that complete neuromuscular block ensues. In mammalian muscle, neuromuscular block can occur as a component in fatigue with voluntary or reflex contractions. It seems most unlikely however that exhaustion of the terminal stores of a transmitter ever occurs in normal circumstances in any of the post-ganglionic transmissions. In the first place the natural frequency of discharge of the involuntary nervous system seems to be comparatively low, and even when the post-ganglionic nerves are stimulated at a frequency causing the maximum liberation of transmitter, long-lasting activity does not cause any serious falling off in the output of transmitter.

With all the cholinergic transmissions I think one can discount the likelihood of accumulation of transmitter at its site of liberation having any significant physiological role. Remarkable effects of accumulation of acetylcholine can be produced if an animal is treated with an anticholinesterase, and accumulation can occur if a tissue is perfused with a saline solution. But in normal circumstances, the presence in tissues innervated by cholinergic nerves of concentrations of true, or specific cholinesterase and the presence in other tissues and the blood of the non-specific or pseudocholinesterase makes a double barrier against accumulation. The concentrations of true cholinesterase which can be specifically stained and are almost diagnostic of a cholinergic junction also make it most unlikely that acetylcholine liberated at a nerve ending can act far away from its point of liberation. This seems to be certainly true for neuro-skeletal muscular transmission. It is probably true for ganglia, although there is some evidence that liberated acetylcholine has actions other than excitation of the ganglion cell. In the heart

again it seems improbable that acetylcholine acts much at a distance from the regions densely supplied with nerves where it is discharged. The other sites of transmission by acetylcholine present a rather different picture. It is certain that the acetylcholine liberated at the ends of nerves causing dilatation of blood vessels diffuses far enough away from its point of discharge to cause contraction of denervated skeletal muscles in the vicinity, denervated muscle being extremely sensitive to acetylcholine. We know far too little about the anatomical arrangement of nerves and smooth muscle; although most organs containing smooth muscle show a nerve net of varying complexity it is difficult to imagine that every smooth muscle cell receives an individual innervation. There is, in fact, much to suggest that in many organs there is a combination of a sparse direct innervation with diffusion of the liberated transmitter to adjacent cells.

So much more is known about cholinergic transmission, because of the existence of two such admirable objects for experiment as skeletal muscle and sympathetic ganglia, than is known about adrenergic nerves that there is a constant temptation to extrapolate the phenomena of cholinergic transmission directly to transmission by adrenaline and noradrenaline. One difference is immediately obvious: if the drug eserine had not been found to protect acetylcholine from enzymatic destruction little would be known about cholinergic transmission. What little we do know about adrenergic transmission has been acquired without the use of a specific inhibitor of an adrenaline-inactivating enzyme. Most tissues contain monoamineoxidase which can inactivate adrenaline and noradrenaline, and it was natural to assume that this enzyme played the part in adrenergic transmission that cholinesterase does in cholinergic, more especially as amineoxidase is found in tissues innervated by adrenergic nerves and diminishes in amount when the nerves are cut and degenerate. There is much evidence to associate amineoxidase with destruction of the adrenergic transmitter, but you will later hear a paper which throws doubt on this view and suggests that adrenaline and noradrenaline are inactivated not by the tissue amineoxidase but by combination with the receptors in the tissues they act upon. This inactivating mechanism can apparently be relatively easily swamped, and liberated noradrenaline escaping into the bloodstream can be collected and estimated. It appears obvious that in the adrenergic system there can be nothing like the localisation of action that occurs in cholinergic systems. Action at a distance from the site of liberation appears to be inevitable. By this I do not mean action in organs far distant; it is unlikely that this can happen ordinarily, but it appears highly probable on the other hand, that within the organ of liberation there is quite widespread diffusion and effect. This idea is not at all incompatible with the anatomical arrangement of the sympathetic system which, as I previously pointed out, is in no way adapted to precision of action.

I have, as I promised, so far confined my remarks to the peripheral nervous system of vertebrates. There is no doubt that chemical transmission takes place in the vertebrate central nervous system and that some, if not all, of the peripheral transmitters are to some extent involved. The indications are, however, that the part they play is subsidiary and that the search must continue for central transmitters other than those so well established for the periphery. The dominating interest for those working on transmission in the central nervous system is the interplay in the nerve cell of excitatory and inhibitory influences. Similar interplay can occur in the post-ganglionic transmissions in the vertebrate as in the heart and in smooth muscle, but nothing closely resembling the phenomena of central inhibition is seen in the rapid cholinergic transmissions to skeletal muscle and ganglion cell. The invertebrate world is opening another fascinating vista of experiment. Here not only are the transmitters unknown, but the muscles of the periphery have excitatory nerves and inhibitory nerves whose transmitters appear to compete

for the muscle receptors in the way that acetylcholine and curarine compete for the vertebrate neuro-muscular junction.

This rapid and inadequate survey will serve I hope to show that in the last thirty years the idea of chemical transmission has wrought a revolution in physiology. The revolution is not yet over, and greatly as our knowledge has increased, so also has the conviction of our ignorance of many of the most important factors governing transmission from nerve cell to effector organ.

THE WORKER AND HIS WORK[1]

ADDRESS BY DR. H. G. MAULE

PRESIDENT OF SECTION J

THE nineteenth century was an age of scientific progress, an age of unprecedented industrial expansion, and an age of individual hardship and personal misery.

Historians have discussed the exploitation of the new class of propertyless industrial worker. They have also described the struggle of the humanitarians against the evil consequences of the unrestricted development of industry.

In starting from this point I will attempt to show that the actions of these nineteenth-century 'men of good will' were in some respect the forerunners of the actions of the *scientifically* enlightened of today. Both are concerned with the well-being of the industrial worker.

The Industrial Revolution brought staggering changes to the ordinary people of England. The new industrial worker had no protection against exploitation by the factory-owners. Not only the hours of work, but the conditions of work, too, paid scant heed to the needs or welfare of the worker. And presumably the employers supposed that their own interests were being served in this way.

As the century progressed, the pressures of public opinion, reinforced by that of legislation, came to the aid of the worker, and his conditions of work generally improved; furthermore, the attitude of certain individual employers was still ahead of public opinion. What then was the effect of these forces on the worker and his work?

In 1845 the hours of work were very largely unlimited and it was not unusual for women to work in the textile factories for twelve hours per day and sixty-nine per week. In this year Mr. Leonard Horner (1), one of the original four appointed Inspectors of Factories, describes how he came across an experiment in the matter of hours of work. It had come to Horner's notice that a textile factory in Preston had made the experiment of reducing working hours from twelve to eleven per day, and that this had excited a good deal of interest in the district.

Horner describes the factory, which had 'steam power equal to eighty horses and employed 568 hands'. The reports reaching Horner claimed that the reduction in hours had been in operation for a year, and that it was declared that 'the same quantity of produce, at the same cost, has been obtained by the master; that all the workers earn the same amount of wages in eleven hours as was done before by the labour of twelve hours'. Horner reflected upon the moral and social evils of conditions in which the workers, from the age of thirteen years to death, had to work for a fourteen and a half hour day and he therefore entered upon the enquiry with the hope that he should find the case clearly made out, but at the same time with the fear and expectation that he should discover some fallacy. He argued that the intelligent mill-owner would have found the optimum speed for the machines consistent with high production and low waste, and that the longer the machines continued at this speed the more they must produce since the worker would not influence the efficiency of the machine. Such reasoning, which seemed perfectly sound to Horner, led him to fear that 'any abridgement of hours must be accompanied by a reduction in the produce'.

Horner went to the mill and conducted the most thorough and painstaking enquiry. He interviewed the managers and examined the books and convinced himself that the claims were substantiated. With true scientific integrity he reported that 'the explanation they gave showed that I had left out an important element, viz.

[1] Address delivered at the Dublin Meeting of the British Association on Thursday, September 5, 1957.

the extent to which the vigilance and attention on the part of the workman can influence the amount of production. They stated, that by greater assiduity on the part of the workpeople while they are at work, by coming punctually at the stated times, and by avoiding needless wastes of time throughout the day, the object was accomplished.'

Finally, Mr. Horner interviewed the workers—not a properly constructed sample as we now understand it, but at least (as he wrote) 'some spinners and some weavers who came to me at my inn'. These workers told Horner of the many advantages and enjoyments they derived from stopping an hour earlier, and informed him that the number attending night school had gone up from twenty-seven to ninety-six since the change. Even with all the evidence before him, Horner thought up alternative explanations for the alleged results and put them to the test. He heard of another mill, that of Messrs. Horricks and Jackson, which had made similar changes, so Horner repeated his enquiries there. On this occasion he quotes verbatim the replies of the spinners, who said: 'We work with more spirit, we have the reward ever before us of getting away sooner at night, and one active and cheerful spirit prevails the whole mill, from the youngest person to the oldest hand, and we can greatly help each other.'

Despite the undoubted success of these experiments, there is little evidence that the example was followed on any large scale.

Turning to 'welfare' in its present-day sense we find a somewhat similar picture. Between 1840 and 1850, reports of the factory inspectors carry descriptions of model villages for the workpeople, the provision of churches and schools, and even in one instance a public wash-house, though this was no more than a 'watercourse for washing and a line of bleach green all along the watercourse' (2). Some employers were, in fact, showing a remarkable awareness of the wider social needs of the worker, and seemed to be of the opinion that these improvements in the social conditions were having some desirable effect on the behaviour of the worker. The inspectors recorded that 'the people (affected by this change) in general are sober, and tippling in the forenoon is a very rare occurrence'.

The factory inspectors recognised the opportunities that were open to them for encouraging good practices, and their function is described by Horner in the following terms: 'The inspectors have it in their power to do much good by availing themselves of the opportunities they possess of making known, in other parts of their districts, plans for improving the comfort and conditions of the workpeople, which they have seen carried successfully into operation, for I am fully persuaded that many masters would gladly promote the welfare of their people if they knew of any practical mode of doing so. It is very natural for them to view with incredulity the practicability of plans by mere good intentions, without being supported by example; but they will take a different view when an instance can be produced of the successful working of arrangements in an establishment similarly circumstanced to their own.' This might be called a plea for science and more factual knowledge to come to the aid of the men of good will, and to influence the behaviour of those not so motivated.

In terms of the human factor this was generally the age of ignorance. The employer was primarily concerned with maximum production. In the excitement of new engineering and technical developments, the worker as a person received scant attention. The notion that the study of people at work might be worth while had not emerged. It was only the few enlightened employers who were aware that such a study of the worker might be as rewarding as the study of his machine, and who acted upon the awareness by paying attention to the worker's general well-being. Nevertheless, the next forty years to 1890 witnessed a slow-moving but general trend towards better working conditions, particularly in respect of the hours of work of women and children.

Apart from this general trend towards better conditions, the next important landmark in the industrialists' growing perception was perhaps in 1893, when Messrs. Mather and Platt conducted their famous experiment on the relation between hours of work and the economic prosperity of the firm. At that time the traditional Lancashire working week in the engineering industry was fifty-four hours, work starting at 6 a.m. Mather reduced the hours to forty-eight, and started at 8 a.m.

The effects of this change were measured with care. An increase in overall production was accompanied by a decrease in lost time (from 2·46 per cent to 0·46 per cent), a slight increase in the ratio of wages to turnover, and a slight decrease in overheads.

Reviewing the results at the end of two years, Sir William Mather said: 'Of this I am assured, that the most economical production is obtained by employing men only so long as they are at their best—when this is passed there is no true economy in their continued work.' Even if Mather's attention seems to be directed at the material advantages accruing to his firm, he is unique in recognising that lost time might be a measure of such advantages and in recognising that the cheerfulness and brightness on the part of the workpeople were significant and that the effects might be measured.

Although the Mather and Platt experiment stimulated a wide interest, its implications received little attention. True, the Government shipyards and arsenals instituted a forty-eight-hour week, but there is no record that any widespread change in practice took place elsewhere. The primitive science of human behaviour did not markedly influence economic or welfare practice.

It was not until the First World War that the next important developments took place. In 1914 (as twenty-five years later in 1939) the country suddenly became aware, in the most alarming circumstances, that its existence depended upon the efficiency of the men and women working in industry.

The production of munitions, and more and more munitions, was essential for survival. In order to meet these demands the reaction of the industrialist was to increase hours of work. In 1915 the position had become desperate, and in September of that year Mr. Lloyd George—Minister of Munitions—appointed a Health of Munition Workers Committee to advise on questions concerning 'industrial fatigue, hours of labour and other matters affecting the personal health and physical efficiency of workers in munition factories'.

In December, Seebohm Rowntree was appointed director of welfare in the ministry, and as the reports of the committee were published his welfare division was given the task of arranging the necessary action.

At last a union had been formed between science, welfare and administration.

The Health of Munition Workers Committee was speedy in action. Within four months it had collected enough data to demonstrate conclusively the adverse effect of excessive hours of work and had published recommendations. By 1918 the final report (4) of the committee was published, together with twenty-one interim reports and memoranda. The topics investigated included welfare, supervision, industrial fatigue and its causes, health and welfare outside the factory, and so on. The foundation of modern industrial psychology had been laid. The way was at last open for the scientist to concern himself with the human problems of people at work.

The years that followed the end of World War I were, in terms of the developing science of industrial psychology, some of the most exciting. The Health of Munition Workers Committee was disbanded at the end of 1917, although its final report did not appear until 1918, and although some of its unfinished enquiries were continuing under the direction of the Ministry of Munitions. The studies of the H.M.W.C. had necessarily been limited to war conditions in industry and

especially to the manufacture of war equipment. Fortunately, however, several influential people and organisations realised that the importance of the work transcended the special conditions of war, and in July 1918 the Industrial Fatigue Research Board was appointed to continue the work already begun.

The growing importance attached to studies of the worker and his work can be assessed from the early reports of the I.F.R.B. The now familiar names of the early pioneers occur with frequency in these reports.

It is perhaps even more interesting to examine the titles of those reports and to see the way in which the patterns that have developed in the years since World War II were clearly and systematically laid down by the workers of the 1920's.

One of the overriding interests at that time was in the problem of the relationships between hours of work and physical conditions on the one hand and industrial efficiency on the other. But this was by no means the only line of development. In 1922, when the third annual report appeared (5), no less than twenty-two official reports had been published, besides a number of articles in the *British Journal of Psychology*, the *Journal of Industrial Hygiene*, and elsewhere.

The names of the authors are now well known, but at that time they were the pioneers and had yet to achieve the eminence that later came to them. Perhaps one may name some of them: H. M. Vernon (6) had reported on hours of work, physical conditions of work and their effect on fatigue and efficiency. Major Greenwood (7) had started his examination of the incidence of industrial accidents which was to lead to twenty-five years of continued studies. Eric Farmer (8) had examined the work of earlier writers on the controversial subjects of time study and motion study, had introduced techniques new to this country, and had directed attention to the importance of ease, rather than speed, of work. C. S. Myers (9) had been concerned with problems of methods of work. In a very short report, he laid down, by implication, one of the principles that has become basic to all subsequent worthwhile studies, namely, that the co-operation of the worker is essential at the outset of any research of this nature. It is remarkable that so short a paper as this could include so many headings which are now the typical chapter headings of any text-book, on the subject: headings such as The Study of Movements—Training—System of Payment—Effect on the Workers, and so on.

The importance of vocational guidance and vocational selection was recognised in two reports in which Muscio (10) was the main contributor.

Wyatt was concerned in four of the first twenty-three reports, and even at this time he was able to point to the extent to which human as opposed to mechanical efficiency might determine production even in operations apparently controlled entirely by the machine. Together with Weston, Wyatt (11) also drew attention to the importance of machine design in relation to psychological and physiological needs.

The possibility of a conflict of interests between the engineer and the psychologist was also implied by Wyatt when he suggested that the tendency to further standardise methods of work was not necessarily advisable 'since the human organism itself does not exactly conform to a rigid type'. Sometimes today this divergence of interest and emphasis has led engineers to forget that human workers have any claim to impose conditions on engineering design, and perhaps in the same way psychologists have failed to acquaint themselves adequately with the problems confronting the engineer.

The last reports I would mention from these early years are studies in the laundry trade, carried out by May Smith (12), and a preliminary study in the boot and shoe industry (13). There is a special reason for naming these two industries, for one was to appoint a psychologist to its trade association in 1935, and to the other a similar appointment was made some twenty years after that.

So far I have only referred to the growth of research promoted by government,

but this was not without its growing-pains. In 1921 the Government was confronted with an industrial and financial crisis for which it was unprepared. In a policy of drastic retrenchment the decision was made to discontinue the financial support given to the I.F.R.B. and to rely on industrial and voluntary support to take its place.

It seemed likely that the work of the Board would be brought to a sudden end, and its staff disbanded. However, acting with unexpected energy, and almost immoderate haste, a group of influential men from all political parties and many branches of industrial and commercial life was able to protest so effectively that the staff were quickly reinstated. Nevertheless, there was to be a severe curtailment in the amount of work undertaken in industry during the years of industrial depression. While these conditions imposed limitations on the investigations carried on in industry, the Annual Report for the year 1922 stated that a large part of the board's work, 'in particular laboratory research which was unaffected by conditions of trade, actually expanded in scope, and definite results, which may eventually have an important bearing on industrial occupations, have already been secured'.

These are modest words to describe the opening stages in a new and developing science.

This same annual report contained six personal contributions from investigators. Examination of these notes would be rewarding to everyone who has any responsibility for some facet of industrial health, production, or efficiency. In particular, the note, by H. C. Weston, on machine design in relation to the operative, might well be taken to heart today by every designer of aircraft, of motor-cars, or of factory machines of all sorts, including those concerned with the current problems of automation.

Weston stressed not only the importance of design in relation to ease and convenience of work, but also the potential relationship between machine design and safety. In his view the growing awareness of the loss to industry represented by accidents was beginning to lead to considerable improvements in machine design. In examining industrial conditions thirty-five years later, we must conclude that though much has been achieved, there is still infinite room for more widespread application of the principles he put forward.

At the same time as the I.F.R.B. was confronted with these economic and policy problems another organisation was emerging from an idea to a reality. In 1921 Dr. C. S. Myers, with the aid of progressive industrial and commercial leaders, started the National Institute of Industrial Psychology. In some ways the work of these two great pioneer organisations ran in parallel and complementary paths for the next twenty years.

Perhaps, as was inevitable by their very nature, the one was more concerned with research and the other with its application. A complete and cordial understanding existed between the two organisations, a point which was mentioned in the annual reports from year to year. From 1922 to 1930 C. S. Myers was a member of the Health Board (the word 'Health' replaced 'Fatigue' in 1928), and throughout the whole of the period between the wars he served on one or more of its committees.

It is hardly practicable to consider individually the part played by these two organisations in the developments that took place between the wars, for not only was there interaction and interrelation between them, but other growing points began to emerge. By 1939 industry was beginning to feel the influence of psychological work, not only through these organisations but from other sources such as the Industrial Welfare Society, the Institute of Personnel Management, and the factory department, whose Chief Inspector, D. R. Wilson, had spent ten years as secretary of the I.H.R.B. By this time, too, the influence of industrial psychology had made a considerable impact and progress in the United States, and the reports of the Hawthorne experiment were beginning to cross the Atlantic in a series of exciting

books. The first of these books appeared in 1933 under the title *The Human Problems of an Industrial Civilisation*, by Elton Mayo (14).

Since the publication of *The Human Problems* there have been so many thousands of words written and so many hundreds of hours of discussion on its content that we may easily tend to forget that when it first appeared it introduced a new direction of thought on the relations between the worker and his work. In its early stages the experiment was designed to investigate the effect of lighting on industrial efficiency. The results of this early work were as puzzling as they were unexpected, but they caused Mayo and his colleagues to set up a new experimental situation in which the performance of a group of six women workers was kept under microscopic examination for a continuous period from March 1927 to July 1932. Throughout this period the actual type of work remained unchanged but changes were made in certain conditions of work.

Very briefly the experiment took the following course. First the active collaboration of the workers was achieved. Next the conditions of work were changed, one at a time—shorter working day, shorter working week, change of length and number of rest pauses, food and refreshment in the morning break. As Mayo describes it: 'Slowly at first, but later with increasing certainty, the output record (used as an index of well-being) mounted. Simultaneously the girls claimed that they felt less fatigued, felt that they were not making any special effort.

'What actually happened was that six individuals became a team, and the team gave itself wholeheartedly and spontaneously to co-operation in the experiment. The consequence was that they felt themselves to be participating freely and without afterthought, and were happy in the knowledge that they were working without coercion from above or limitation from below.'

The real significance of this study is that it brought dramatically into the limelight the significance of the organisation of working teams and importance of free participation of such teams in the task and purpose of the enterprise for which they were working as it directly affects them in their daily lives.

The more immediate effect was to encourage the research workers and the heads of the company to widen and extend their studies in two directions. One of these, the studies in the bank wiring room were fairly clearly defined and precise. The other, the interview programme, more diffuse.

If the bank wiring room experiment showed nothing else, two lessons emerge. First, that deep psychological forces, feelings and anxieties may be more significant in the restriction of production than the desire to increase weekly earnings by greater physical effort. It is easy to be rational and to condemn the more obvious manifestations of 'restrictive practices' adopted by other people. Moralising and blaming or appealing to reason, however, are poor techniques with which to attempt to alter behaviour, the roots of which are too deep to be touched at such an intellectual and superficial level.

Secondly, the study revealed some of the subtle informal organisation that took place within the group. These informal organisations controlled and regulated the behaviour of the group and had the effect of acting as a break whenever there was any threat of change.

It is apparent that there is a marked difference between the general nature of the findings in the relay assembly study and those in the bank wiring room. The reason for the contrast is perhaps a sixty-four dollar question for contemporary industrial management.

Despite the criticism that has in recent years been levelled against the Hawthorne studies, some of its lessons are important and should not be ignored.

In their short account of the experiment published in 1948, Urwick and Brech (15) suggest that industrial management, if it is to achieve the psychological conditions appropriate for the realisation of its objectives, must accept and implement three

principles. The first requires a system of communication and consultation between management and worker; the second extends this into genuine two-way consultation on all matters of management. The third requires the development of adequate training for those with managerial or supervisory responsibility in industry, and states that such managerial training should make particular reference to interpersonal and social relationships within a working community. At this point, we may add in parenthesis that the last twenty years have seen considerable development of these ideas throughout British industry. Each one of them had been adopted by individual firms long before the Hawthorne studies.

In his first book, Elton Mayo goes to considerable trouble to make it clear that he himself and his colleagues were largely inspired by and deeply indebted to the British studies and but for this early work the knowledge and climate of opinion in which a 'Hawthorne' study could take place might not have existed. The Hawthorne studies redirected and stimulated thought in this country, but it is probably equally true that but for the background provided by the British work in the preceding ten years the Hawthorne studies might not have taken place. The advantage that the Americans had over their English colleagues was the unstinted financial support of a wealthy and enlightened firm that was prepared to be its own guinea-pig. The Hawthorne experiment ended with the economic depression of the 1930's, when the experimental rooms were closed down and the research workers were left to study their results and write up their findings.

By the time World War II broke out, the Industrial Health Research Board had published eighty-four reports on field and laboratory studies in addition to its annual reports. It had offered to industry a quantity of factual information and a rich diet of new ideas about the way that workers thought, felt and behaved in their working life. It had offered many challenges to industry which, by and large, industry had not taken up. But, in the form of questions, few industrial firms would have been able to give an answer on the basis of anything more substantial than 'hunch'.

The following questions might have been used as a challenge to managers and to indicate to them that there was plenty of scope for new directions in management thinking.

To the manager of a fine assembly shop: Why do you work your present arrangement of hours? Have you ever thought of shorter spells of work with additional rest breaks in the working spell? Or: Do you know that would happen if you allowed workers in a team from time to time to exchange their actual operation with that of one of their team mates?

To the manager of an engineering works: Why do your workers do this job in this way? Have you ever examined the details of the job and measured the time wasted through delays and hold-ups? How do such hold-ups affect the workers? Are they frustrated by them, or do they welcome the chance to take a rest?

To the manager of a printing works: Have you thought about the difficulty with which your type-setters have to contend on account of the low intensity of illumination and the poor arrangement of lights?

To a laundry proprietor: You complain about the errors in packing and sorting. Is it possible that some of your workers are working at tasks inappropriate to their skills and are more suited to different tasks?

And to the manager of any factory or office: What is the basis on which you promote your workers? When you have promoted them, what do you do to train the promoted man or woman in his new responsibilities?

And finally, one could have asked: Do you know how much time is lost through sickness and other absence? Do you know what is the cause of such absence? Can some of it be due to the way the worker feels about his work or the way he feels about *you*?

These are only a few of the possible questions. In discussing each one with the manager the psychologist could have referred to some work that had been carried out and reported. If the manager was a man of courage and imagination he would quickly see that there were many ideas to exchange and many new ways in which he might usefully think and act.

Psychology and the psychologist were offering their service to industry. On the whole, industry was too busy to accept the offer.

There may be some doubt as to how far current publications form an accurate barometer of current work and current interests. It seems reasonable to suppose that any serious professional or learned society's journal tends to publish articles of contemporary interest and to reflect the directions in which its subject is progressing. If this is true, it is with some surprise that one examines the titles of articles published in *Occupational Psychology* in 1939 (16). Twenty-four major articles appeared in that year. Ten of them were concerned with vocational guidance, vocational selection, or closely related topics; five were general surveys of work in other countries—U.S.A., Spain, Italy; two were very general discussions on the health of the industrial worker; two dealt with aspects of intelligence and its testing. Apart from those touching on selection, only two could really be described as being concerned with the direct application of psychological studies to the industrial worker and his work in Great Britain.

Examination of individual articles reveals not only the passing of one epoch but also the shape of things to come. The editor introduced an article entitled 'The Servant Problem' as follows: 'More women enter domestic service than any other occupation. In this article Dr. Myers describes the causes of the present defective quality and quantity of domestic servants.' A passing era indeed! The things to come are only too realistically forecast in an article in the July number which stated: 'Generally it is possible to say that the results obtained during the Spanish War by a very small band of trained psychologists working under the worst imaginable conditions does definitely suggest that the thorough application of psychotechnical principles would be of very great use in event of another war.' And so to the echo of these words the country plunged into the greatest cataclysm of history.

It has been said that many of the great scientific and social advances of the present century have taken place under the shadow and with the stimulus of war. The Interdepartmental Committee on Physical Deterioration which reported in 1904 (3) was the direct outcome of the experiences of the Boer War, and led, directly or indirectly, to the School Medical Service, the medical examination of young people in industry and to many other later features of the health services of this country.

The 1914–1918 war caused, or at least speeded up, the transformation of the science and technology of air transport from a primitive Meccano-and-string era to the grown up science that it soon became. Similarly in World War II.

In 1939, psychology and psychiatry in the service of the armed forces was little more than a distant memory from the First World War. The scope and pattern has already been indicated. What was it that emerged after five and a half years of war?

Gradually the Services began to believe that psychology might have a part to play in ensuring that the right Service man (or woman) was engaged in the right Service occupation with the right tools. The problems of the worker and his work are not fundamentally different when the worker is put in uniform and called a soldier and when the work becomes the prosecution of military tasks against an enemy. His machines are no longer the lathe, the press and the jig-borer. They become the tank, the radar equipment and the aircraft. The need for the machines of war to be so constructed that the soldier can operate them easily and accurately is more compelling, the penalty of bad design may be a national disaster. And so the failure of the traditional approach and the pressure for national survival combined with the other forces and led to the establishment of substantial and influential bodies of men and

women applying existing, and seeking new, psychological knowledge about the conditions favourable to the worker being effective in his work.

Not least of the problems that were tackled, and in the tackling of which some degree of success can be claimed, was that of deciding which recruit should be allocated to and trained for which job. This work, together with the related problems of officer selection, have since the war been fully described by Vernon and Parry (17) and others.

When the psychological departments of the armed forces were first started, the necessary staff was available only because, on the whole, industry was not employing them; industry considered that it had matters to attend to more important than those requiring the skills of the psychologist.

Unfortunately the number of trained people available was very limited. In the period between the wars there had not been sufficient interest in such matters to ensure an adequate supply. Thus, the small number of experienced people in each service had not only to create the machinery but also to find and train a sufficient staff to make possible the efficient working of that machine. In this they were by no means unsuccessful, and at the end of the war there were probably more people than ever before who had some experience in exploring and tackling the problems of the relationship between the worker and his work.

We have come at last to the post-world-war period. During these last twelve years since 1945 there has been an energetic revival of interest and activity in the industrial field. New organisations have come into being and new research projects started. There has been a steady flow of reports and publications too numerous to mention. All I would attempt is to indicate briefly a few of the main lines of advance.

Of the various developments that have taken place, I would particularly turn to those that are concerned with the problems of attitudes to work and satisfactions in work. The particular contributions that I will draw upon are those of Mrs. Raphael (18), of the National Institute of Industrial Psychology, and Dr. Wyatt and his colleagues (19) of the Medical Research Council. In addition to these workers, however, many others have made studies or expressed views on these important questions. The various methods now adopted for the investigation of attitudes are too well known to require description. The findings in industry, however, are less well known than they deserve. The stresses and strains that exist within working groups and that deprive the worker from realising satisfaction from his work are often, at least in part, within the control of management.

The first and most important need is a general realisation that to the individual worker the experiences of working life are infinitely more than a simple relationship between individual and task.

A man or woman may or may not find satisfaction from his particular task, and it is important that, so far as possible, the actual task should at least not be uncongenial to him. But his total experience of a satisfactory working life is influenced by many other things.

The cost to the individual and to the community of the worker being out of harmony cannot easily be measured, but we have some facts and some estimates of this cost. The Minister of Labour has given figures for the working time actually lost to industry through industrial disputes, sickness and industrial accidents per year. For industrial disputes the figure given was $3\frac{3}{4}$ million man-days, for accidents 18 million man-days, and for sickness 280 million man-days. The losses through sickness are about seventy-five times our losses through industrial disputes. At any given time about 1 million men are away from work through sickness.

These are startling figures, but to this information we must add additional facts. A study of neurosis in industry by Fraser (20) has shown that probably between one-third and one-quarter of all sickness causing industrial absence can be attributed to psychological rather than physical causes; though it may not be easy to prove, we

may suspect that some of the labour disputes (that receive so much more publicity than the less sensational facts about general health in industry) also have their deeper roots in psychological strains. As a nation we are paying a heavy economic price for maladjustments which perhaps would be amenable to investigation and treatment.

Another authority has said that the cost of people changing from one job to another is around £700 million per year. If only 10 per cent of this could be avoided, it would be a considerable saving to the nation. Those who have attempted to study these problems would not claim to be able to give a prescription for cure, but at least they believe they know some of the causes of stress which contribute to the disease. Much more might be done, for example, to ensure that people are engaged in tasks appropriate to their intellectual skill, quite apart from other aspects of their total make up. In his presidential address to this Section in 1953, Harding (21) drew attention to the waste that could occur through failure to recognise great capacities and the devastating effect that this could have on the happiness and well-being of the victim. For every one who suffers such a catastrophe, there may be many more suffering impairment of happiness and efficiency, thwarted and restive in inappropriate occupations.

The trend towards a more sensible approach to vocational selection and vocational guidance, though gaining momentum, is still slow. The collection of more knowledge is hamstrung by the lack of sufficient funds; the application of existing knowledge is curtailed by absence of serious interest. Whether it is the skilled or unskilled worker at the bench, the manager, or the executive, the cost of inappropriate placement is seldom assessed. The cost of wrong promotion may be far greater than the direct results stemming from the inadequacy of wrongly promoted individuals. Mrs. Raphael has described the thwarted feelings that exist where people feel that the criteria for promotion are capricious and unjust. The studies of Likert (22) and others in America are directing managers' attention to the limitations of what has been called the 'scientific management' approach in management techniques. In its place there is a wider acceptance, at least in theory, of the desirability of thinking of management in terms of human dynamics and in terms of the interaction of individual on group and group on individual. The swing towards a socio-psychological orientation in management thinking is an important and hopeful sign.

In their most recent publication of attitudes to work, Wyatt and others have shown how difficult it is to separate satisfaction in the work task itself from the wider satisfaction in the total working situation. The workers studied were largely engaged on simple semi-skilled or unskilled operations, but a quarter of the men described themselves as very satisfied with their actual work and another 44 per cent were moderately satisfied. Less than one in ten were dissatisfied; the subtle effect of social relationships on a man's total orientation to working life is described: 'When a man joins a stream of workers going through a factory gate he loses some of his individuality and becomes a member of the factory community. When he reaches his bench he becomes an integral part of a smaller or more compact group, and reacts to group feelings and behaviour. He derives comfort from the presence of others doing the same kind of work; a task which is intrinsically distasteful and monotonous may become acceptable and even mildly enjoyable under the subtle influence of group comradeship.' These are influences about which management has been insufficiently aware or has been too ready to disregard.

One hundred and eleven years ago, Macaulay, speaking in the House of Commons on the Ten Hours Bill, made a moving plea for a deeper understanding of human needs. He was thinking in terms of physical needs, the need for rest and recovery from fatigue, and speaking specifically of the need for Sunday as a day of rest from industrial work when he said: 'Man, the machine of machines, the machine compared with which all the contrivances of the Watts and the Arkwrights are worthless, is repairing and winding up, so that he returns to his labours on the

Monday with clearer intellect, with livelier spirits, with renewed corporal vigour. Never will I believe that what makes a population stronger, and healthier, and wiser, and better, can ultimately make it poorer.'

Today we have gone a long way in our understanding of man's need for a physiologically appropriate environment. These, the problems that were recognised as most urgent nearly half a century ago, are now largely understood and their solution now generally accepted. Today we are only at the beginning of our search for the psychological needs of men at work. But a beginning has been made. Industrial management should at last recognise, and act upon the recognition, that the psychologist and sociologist have made and can continue to make a real contribution towards understanding the facts which are essential if a healthy, efficient and satisfied industrial community is to be achieved.

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ASPECTS AND PROBLEMS OF CONIFER EMBRYOLOGY[1]

ADDRESS BY PROFESSOR J. DOYLE

PRESIDENT OF SECTION K

THE future development of this great Association now meeting in Dublin and the part which it can best play in furthering the broad interests of science in general have been, as we know, under active discussion. Clearly the status of science in the social framework of 1957 is a very different thing from that of a century and a quarter ago when, in September 1831, the Association was established in York. There was need then for the vision and high aims of the founders who proposed an Association to give a stronger impulse and more systematic direction to scientific enquiry by pointing out, at annual meetings in various centres, the lines of direction in which the researches of science should move, the particulars most immediately demanding attention, the problems to be solved, the data to be fixed—in fine, to assign to every class of mind a definite task! Obviously a literal application of such proposals is hardly possible today.

It is of course in no sense a function of this address to consider the lines along which development might be most usefully steered, but it should be said that nothing can lessen the importance of the work of the past no matter what the future growth may be. The Annual Reports of the Association are now documents of great interest in which can be read the history of scientific development during a period of most active change. They are moreover a mine of fascinating information on a variety of other topics—the first appearance of great personalities, the nature of current social and economic problems, even the folly of over-confidence in scientific theory. To get the impression one need delve no farther than the Reports of the previous Dublin meetings in 1835, 1857, 1878, and 1908. Take for example the meeting in 1857. There can be found a paper on a proposed canal through the Isthmus of Suez expressing grave doubts on the feasibility of the project. There too one can learn with interest that a certain Rev. Dr. Livingstone gave an address on his recent discoveries in Africa. A galaxy too of Irish personalities, many of whose names are still part of our tradition, were prominent at that meeting. William Rowan Hamilton, the mathematician, was there as a Vice-President, and we find too such names as Wilde, archeologist and Victorian doctor, Samuel Ferguson, John O'Donovan, and William K. Sullivan the chemist, while Callan of Maynooth spoke on his now famous electro-dynamic induction coil. On the biological side, Harvey, the algologist, presided over the Section, then Section D, while among the various special committees appointed to make reports was one which included such historical names as Babington, Huxley, Hooker, and Berkeley. Such is the kind of general interest which these Reports provide.

If we look more specifically at our own subject it is distressingly interesting to see what a subservient position it held for more than two generations against chemistry or geology, or even anthropology. Botany was always linked in those earlier days to one or two other subjects, and that not to its advantage. Thus, turning again to the 1857 meeting, we find that botany, in which only eight papers were read, was included in Section D with zoology, in which seventeen contributions were made, and with physiology in which fifteen papers were given. In contrast, geology was a separate Section in which alone there was a total of thirty-eight addresses. The relative position of botany at the 1878 meeting was even worse. From this time

[1] Address delivered at the Dublin Meeting of the British Association on September 5, 1957.

onward, however, the pressure of botanical unrest steadily increased, and in 1895, but not till then, we achieved our independence and Section K was born. This is not the place to seek an explanation of so peculiarly tedious a gestation, but, at this the second meeting of Section K in Dublin, it may not be out of place to remember our struggling beginnings. K* (Forestry) of course is still but a child, not having been born till 1925.

It would then be possible, and interesting, to follow from these Reports the historical development of our subject; to compare, for instance, Professor Daubeny's remarks at the 1835 Dublin meeting on the action of light on plants with the contributions made by Blackman, Thoday and others at the session on photosynthesis in 1908; to observe the early struggling appreciation of the importance of Mendelism; to watch the growth of ecology; or to note the first references to viruses in plants. In the Association's Reports there stand out, as in a modern documentary, the highlights, the important phases, of our botanical history. A tribute is due to their value and interest.

Apart however from these more general considerations, Irish floristic botany owes a direct debt to the British Association, a debt which should be acknowledged at this meeting. At the 1835 Dublin meeting we find a heavy burden placed by the Committee of Section D on the shoulders of one James Townshend Mackay. He was asked among other things to determine the limits of the species of *Saxifraga*, especially those native to Ireland, a problem with a very modern ring. He was also asked to report in general on the vegetation of Ireland, and undoubtedly it was this support which encouraged him to complete and publish his *Flora Hibernica*. Thirty years later the Association, at its meeting in 1865, gave a substantial money grant towards the publication in 1866 of Moore and More's *Cybele Hibernica*. These were standard works on Irish floristics up to Praeger's time. Again, at the last Dublin meeting in 1908, Section K initiated the formation of a committee to support the scheme then being locally organised for a survey of Clare Island and to administer a money grant allocated by the Association. This Clare Island Committee consisted of Professors T. Johnson and Grenville Cole, Dr. R. F. Scharff, A. G. Tansley, and R. Lloyd Praeger. Much could be said, if time allowed, in praise and in memory of each of these notable names, as well as the members of the local committee—Colgan, Lett and Barrington. One cannot however refrain from paying a special tribute to the memory of one of them—Robert Lloyd Praeger, our greatest Irish field botanist. That tribute must be all the more sincere when we recall that he left his estate in trust to the Royal Irish Academy for the furthering of studies on field natural history.

Now a president's most difficult task is, I think, not the writing of an address but the choice of a subject upon which to write. Browsing through these interesting Reports, I was at one time tempted to frame this address round that idea of the history of botany in these islands as reflected in the Reports of the British Association. I decided against it for many reasons, but mainly because it seemed clear, at any rate to me, that there could be only one excuse for selecting me for the great honour of being President of Section K for this meeting. It could only be because I had contributed in some small way to our knowledge of conifer embryology, and I felt therefore that my choice was thus already determined. But in so choosing to deal with some aspects and problems of conifer embryology I can hardly claim to be following the aims of the higher executive of the Association. This subject can hardly serve as a text for the rest of the Section's programme, nor can it claim greatly to advance the layman's appreciation of the importance of science today in human affairs. However, the Section as a whole is not neglecting its duty in these matters. Either alone, or in joint session, it proposes to discuss such issues as modern developments in photosynthesis, grassland problems, and biological radiation, to mention but a few.

But even my own choice is perhaps not entirely divorced from these issues.

After all, the conifers are a group of fundamental importance to man. They make a major contribution to the forest vegetation of the world and thus supply the main bulk of the world's timber, valuable not only directly but also as the raw material of so many modern products—*das universal Rohstoffe* as the Germans called it. Indeed, the greater part of the activity of our Sub-section K* is focused on conifers since forestry in these islands, as in most non-tropical areas, is largely the forestry of conifers. Any aspect of structure, growth or development in so important a group of plants can justly claim our interest.

A young student approaching conifer embryology for the first time might well be excused if he got the impression that the matter was somewhat confused and perhaps a little complicated. Schnarf, for example, in his monograph *Embryologie der Gymnospermen*, actually says that 'the development shows so great a variability that it is hardly possible to present a general scheme of its course which is valid and acceptable'. In fact the position seems to me quite otherwise. There seems rather to be in the various families a remarkable uniformity, particularly in early embryogeny, which can be resolved into a few modifications of a basal plan with but a very limited number of exceptions which present any difficulty in fitting into the scheme.

Further, it seems to me that the value of our approach to questions of conifer embryology suffered historically from the circumstance that the material used by the earlier workers was North Temperate material with an overemphasis on the common pine. From the days of Robert Brown, who seems to have been the first to record in writing, in 1834, the occurrence of polyembryony, we find all the older workers contributing to our knowledge of the pine—Mirbel and Spach, who in 1843 first used the terms rosette and suspensor; Schleiden, in 1849; Schact, in 1850, who still thought the pollen tube gave rise directly to the proembryo but who showed that four was the number of cells in each of the proembryo tiers; Hofmeister too and Strasburger; and in more recent times Chamberlain and Buchholz. If the approach to conifer embryology had started in the southern hemisphere, if investigators had begun with the podocarps instead of the pines, we might have got more rapidly to a simple picture. This idea that botanical development in general has suffered from the dominance of investigations on North Temperate types is of course not new. It has often been expressed, and perhaps with greatest vehemence, by students of the angiosperms such as Professor Good and Dr. Corner.

The course of development in any conifer or taxad can be conveniently divided into the pre-fertilisation and the post-fertilisation stages. Since special interest most often attaches to the latter, it is useful for the time being to include these specifically under the term *embryogeny* and to keep the general term *embryology* for the whole course of development. In this short address mainly aspects of embryogeny will be considered.

If then we wish to appreciate the nature of the basal plan of conifer embryogeny, *Pinus* must be set aside and, most conveniently, an approach made through a brief examination of embryogeny in such a simple podocarp as *Podocarpus andinus*. After fertilisation the sporophytic fusion nucleus undergoes five free mitoses, two *in situ* and three at the archegonium base to which the products of the first two have migrated. There results thirty-two free nuclei which become suitably organised and in connection with which membranes are formed to give below a storied group of primary embryo cells and, above, a single tier, the *open* tier, in which the cells remain open to the archegonium. This is followed by a sixth more or less simultaneous mitosis in all the nuclei—the *internal* division. In the *open* tier the spindles of this division lie along the axis line of the proembryo and give the two distinct tiers —the *upper* or *U* tier, commonly open above and ephemeral, and the *S* tier of cells, equal in number, which elongate to give the primary suspensor. Incidentally, in the podocarps, the nuclear mitoses in the primary embryo cells at this stage do not

duplicate the cell number but produce a binucleate condition in each of these cells. This, however, is a peculiarity of the podocarps and does not for the moment concern us, the point of interest rather being that the internal division affects all the cells, the primary embryo cells included. In every podocarp so far followed in detail the sequence is the same, and those others, in which the development is even partially known, are most readily interpreted along the same lines. It is to be noted, however, a point to be returned to shortly, that in most of the podocarps there are only four free mitoses giving sixteen free nuclei. This dropping of the last or fifth mitosis affects in no way the general sequence. Except for the lesser number of cells a similar proembryo, a proembryo with its *U*, *S* and embryo tiers, is formed and formed in a similar way.

There is a strong probability that the conifers have been developing independently in the northern and southern hemispheres perhaps since early Mesozoic time. Provisionally we can visualise the podocarps arising in the deep South and mainly separated for long ages from the northern forms by the great Tethys Sea. Presumably these podocarps had roots in some less differentiated earlier stock at least broadly similar to the basal stock from which the northern types arose. Comparisons then between the southern forms and the northern separated types, such as the taxads, should be of interest. The comparison is in fact rewarding, since we find that embryogeny in *Taxus* is exactly that already outlined for the podocarps. Four free mitoses, producing sixteen nuclei arranged at membrane formation in an open tier above and a lower embryo group, are followed by an internal division which gives the upper *U* tier, the middle *S* tier, and which, in this case, duplicates the primary embryo cells. In all conifers, other than the podocarps, which follow the basal embryogenic plan the internal division, as here, multiplies the embryo cell number. Thus, apart from aspects such as the relative number of embryo and suspensor cells, aspects for the moment of lesser importance, the embryogeny in *Taxus* follows the common plan in podocarps. This is also the plan in *Austrotaxus*, probably a rather archaic type, and in *Amentotaxus*. If we separate the taxads as a special order, and on this question I think we must follow Professor Florin, *Cephalotaxus* would represent a special family of the Coniferales proper, and it lends further interest to these comparisons that this northern form follows too the plan of the taxads and podocarps. *Sciadopitys*, doubtfully of taxodinean affinity but certainly widely removed from the taxads or the podocarps, shows not only the same plan but the five free mitoses and the thirty-two free nuclei of the simpler podocarps.

On such comparisons as these it seems reasonable to infer that this plan in early embryogeny is in fact basal in the conifers. In more primitive types it may have been coarser and less precise than it is in most modern conifers. It may have been perhaps more like that now shown in *Ginkgo*, but it is suggested that the more clean-cut organisation producing a proembryo of the standard $U : S : E$ type was developed comparatively early in the roots from which the modern types sprang.

If then we take something like the *Sciadopitys–Taxus–Podocarpus* plan of early embryogeny as basal in the conifers, we can readily relate the other embryogenies to it. We seem to see simply the existence of a limited number of definite evolutionary tendencies whose expression could produce a certain limited number of modifications of the basal plan, modifications which are in fact those occurring in the group. It is not possible to discuss all these evolutionary tendencies *in extenso*, but perhaps the most obvious of them, the one which most readily illustrates the point, is the shortening of the free nuclear phase by the dropping of one or more of the early free mitoses so that a larger number of free nuclei would appear to be in general a conservative condition, a lesser number an advanced condition. Originally the number of free mitoses may, of course, have been more than five. High numbers of free nuclei, up to 1024 from ten free mitoses for instance, seem common enough in

the cycads. If too the embryogeny of *Ginkgo* is as conservative as its fertilisation, it is of interest that eight free mitoses are shown by it while six occur in the Araucariaceae. In *Sciadopitys* and in the simpler *Podocarpus* species, such as those included in *Stachycarpus* and *Nagaeia*, the number is five. In most podocarps, including all the higher ones, in most taxads, and in *Cephalotaxus*, the fifth mitosis has been dropped, apparently quite independently in each group, so that their proembryos are built up on the products of four free mitoses. In the Cupressaceae, the Taxodiaceae and also, but again obviously separately, in the Pinaceae, only three free mitoses is the normal number. This tendency to reduction is expressed still further within certain of the families. Thus, in the taxads, *Torreya*, commonly recognised as a more advanced form systematically than, say, *Austrotaxus*, has, instead of the four free mitoses typical of the family, only two free mitoses giving normally a four-celled proembryo. It is of interest in this connection that three free mitoses, giving eight free nuclei, have been claimed for certain forms of *Torreya nucifera*, thus bridging the gap between the sixteen-nucleate *Austrotaxus–Taxus* type and the four-nucleate type. Similarly, in the Cupressaceae the number of free mitoses is likewise reduced to two in the South American *Fitzroya*, the Australian callitroids *Callitris* and *Actinostrobus*, and probably also as well in the South African *Widdringtonia*. This reduction is paralleled in the Taxodiaceae in the Tasmanian *Athrotaxis*. In all these cases a four-celled proembryo is formed. The extreme of this reduction tendency is shown of course in the derivative *Sequoia sempervirens* in which membrane formation follows directly on the first mitosis giving at once a two-celled stage.

This shortening of the free nuclear phase seems thus to be a general trend expressed without regard to systematic affinity. Whatever connection may exist between the Cupressaceae and the Taxodiaceae, there can be little close affinity between them and the Pinaceae. The reduction to eight free nuclei must have occurred independently. More obviously, if we take those cases in which the reduction has gone a further stage to give a four-nucleate proembryo, what connection can be suggested between the northern *Torreya* and the southern *Athrotaxis* or between them and the callitroids? The reduction from sixteen to four or from eight to four must again have occurred independently in the three families represented by these respective types. Again, among the typical northern taxodineans, *Sequoia sempervirens*, being a polyploid and almost certainly a hexaploid, must be considered a derivative type. As the other *Sequoia* sp., *S. gigantea*, shows the typical eight free nuclei in common with its other northern relatives, it would appear that *S. sempervirens* represents still another line in which reduction occurred, in this case to its fullest limit. It is perhaps then of some interest to recall that in *Athrotaxis selaginoides*, as a very rare phenomenon, similar two-celled proembryos may occur, although the southern and northern taxodineans have in all probability developed separately since early Mesozoic time.

We will return shortly to this idea, the occurrence in conifer embryogeny of definite trends not related, or at least not obviously related, to systematic affinities. But we must first look at another aspect of early embryogeny in general. The four-celled and two-celled conditions raise minor difficulties which time does not permit us to consider here. The early embryogeny on the other hand of those members of both the Taxodiaceae and Cupressaceae with three free mitoses adheres strictly to the basal conifer plan giving the characteristic conifer proembryo of the $U : S : E$ type. Dropping of free mitoses has not affected the plan or the sequence. *Taxodium* or *Thuja* or *Cryptomeria* only differ from *Sciadopitys* or *Podocarpus andinus* in the lesser number of cells in the proembryo as a whole or in its separate tiers.

Approaching then early conifer embryogeny by the gateway of the podocarps we arrive at a simple picture. Ignoring for the moment the derivative and specialised four- and two-celled proembryos, we find a uniform type of early embryogeny in the podocarps, the taxads, *Cephalotaxus*, *Sciadopitys*, the taxodineans, and the

cypresses and their allies. It shows (*a*) a free nuclear phase; (*b*) a stage in which these nuclei take up organised positions as a single upper tier and a lower group; (*c*) membrane formation giving the single *open* tier above and a group of primary embryo cells below; (*d*) an internal division affecting all the cells, duplicating the embryo cells (or in the podocarps giving binucleate cells) and producing from the *open* tier, the *upper* tier *U* and the middle *suspensor* tier *S*. The only major differences arise from the variation in the number of the first free mitoses and the resulting free nuclei. These may vary from thirty-two (occasionally sixty-four) to eight, but the plan is the same in all. Further, in all, the *S* tier subsequently elongates *in fact* as a functioning primary suspensor. Resulting from a mistaken interpretation of the nature of the embryogeny of *Pinus*, Buchholz referred to this suspensor in these types as the *pro*-suspensor as if it were in some way a *substitute* suspensor, whereas in fact it is the direct organisation of a necessary part of a developing embryo.

Two families, however, the Pinaceae and the Araucariaceae, show departures from this basal plan. The araucarians are admittedly a bit of a puzzle whose solution will not be here attempted. Since, however, *Pinus* is drilled into every elementary student of botany even at school, it may be of interest to examine briefly the position shown by it, because *in detail*, early embryogeny in *Pinus*, as in other members of the Pinaceae, cannot be taken as typical of conifers. Properly understood, however, it can be interpreted in terms of the simple basal plan.

In *Pinus* then, the fusion nucleus, after fertilisation, divides twice *in situ*, the resulting four free nuclei passing to the archegonial base. Subsequent developments, as in other members of the family, seem to be dominated by the simple circumstance that these four nuclei come to lie definitely in one plane. The third mitosis follows to give at membrane formation two superimposed tiers with four cells in each. The upper of these is of course the *open* tier of the other conifers, the lower, the primary embryo group, lying in one plane. The typical internal division then ensues. The open tier divides quite normally into an uppermost tier open to the archegonium and a cellular tier just below it. If there is anything in comparisons, these are respectively the upper *U* tier and the suspensor *S* tier of the podocarps, taxads and others. This internal division also effects the four primary embryo cells just as it does in other conifers. In the pines, however, the mitoses in these primary embryo cells are vertical, and thus, since the four primary cells lie so definitely in one horizontal plane, what appears to be another tier is formed. The eight cells of the two tiers are however all part of a normal embryo group which perhaps is better visualised as four vertical groups of two-celled units. At this stage the *Pinus* proembryo formula is simply $U4\ S4\ E4 + 4$ and, except for the regular grouping in fours, seems to differ in no way from the normal conifer plan.

The pines differ however from those already considered in one important feature. The primary suspensor tier *S* normally does not elongate or function as a suspensor but remains a group of four more or less quiescent cells. It is the upper of the embryo tiers which in fact elongates and takes on the suspensor function. The suppression of functioning of the normal *S* tier has not suppressed the necessity for suspensor activity, and the elongation of the next succeeding tier, the upper embryo tier, gives *it* the nature of a *substitute* suspensor. If the term is used at all, it is this tier which should be called the pro-suspensor, or the substitute suspensor. From another point of view we are here dealing with the elongation of the upper cell in each of the four vertical two-celled units which compose the *E* group. There is nothing really unique in such behaviour. Examination of the course of cleavage polyembryony in certain conifers with normal primary suspensor elongation shows cases, such as *Biota* or *Sequoia*, in which embryo cells behave in a somewhat similar way in addition to the elongation of the primary *S* tier. The cases in question are cases of special advanced types of cleavage so that the phenomenon in *Pinus* is probably equally specialised but it essentially differs from these others only in the

fact that the arrangement happens to be such in *Pinus* that the four elongating embryonic cells lie in one plane and elongate together. It is of further interest to note that in a small percentage of cases in *Pinus* the primary suspensor tier *S* does elongate in the normal way.

Early embryogeny in the pines must thus be interpreted as a specialised derivative of the basal type. Since the days of Mirbel and Spach, however, we have suffered under their nomenclature since it was they who first called the *S* tier the rosette and upper embryo tier the suspensor. In recent times it has been particularly the late Professor Buchholz in America who, over many years, advanced with vigour the thesis that *Pinus* embryogeny and *Pinus* cleavage, or conditions closely akin to them, were basal in conifers and who further sought to interpret the embryogeny of other conifers in terms of that of *Pinus*. This of course was an impossible task, and the greatest stumbling-block was this so-called 'rosette' tier of *Pinus*, the difficulty of demonstrating a corresponding tier in the proembryos in other families. The occurrence of any small walled cells above elongating suspensor-like cells was welcomed and adduced in support of the correspondence. Small walled cells undoubtedly may occur in this position even in those conifers whose development is essentially of the normal podocarp-taxad type. The cells of the *U* tier, the upper tier proper, though commonly open to the archegonium above and frequently ephemeral, may be closed in whole or in part and semi-permanent; where the *S* cells, the primary suspensor cells proper, are numerous, as in many podocarps, some occasionally may not elongate and may be left behind their developing fellows; it is possible, though not yet conclusively shown, that, again occasionally, small cells even may be cut off above from functioning *S* cells. But none of these erratic occurrences of varied origin have anything to do with the 'rosette' tier proper of the Pinaceae, a tier which is an integral morphological part of their embryo. It would seem that new terms might reasonably be coined for use with the latter type if we wish to emphasise the real nature of the basal conifer proembryo and to avoid confusion between it and the derivative *Pinus* type.

We have seen then how, starting with the podocarps, we can come quite simply to the idea of a basal type of conifer embryo. We have called attention to the existence of one general evolutionary trend affecting early embryogeny, the shortening of the free nuclear phase. We have now seen that the early embryony of *Pinus* is not typical of conifers or taxads but is a special variant limited to one family.

Another major trend in conifer embryogeny, but expressed at a slightly later stage, is the occurrence of cleavage polyembryony, a rare derivative phenomenon in angiosperms but a widespread one in conifers. Any adequate treatment of this problem is out of the question here but it cannot be completely ignored. Briefly and dogmatically then, comparisons again between the southern podocarps and the northern taxads seem to show that cleavage is a derivative phenomenon which has arisen separately in the families. Thus in the podocarps there is no cleavage in those forms whose gametophytes and embryogeny are of a less specialised type, such as *Saxe-Gothaea* and *Phyllocladus*, or which are in general less advanced systematically, such as the *Stachycarpus* group of species in *Podocarpus*. Cleavage is a characteristic however of the more advanced or specialised forms such as *Eupodocarpus* or *Phaerosphaera*. In the Taxales similarly there is no cleavage in *Austrotaxus* or *Taxus*, but it occurs in *Torreya*, commonly accepted as a more advanced genus than the other two. Again here we have the interesting point that the type of cleavage shown by the northern *Torreya* is essentially similar to that shown by the southern *Eupodocarpus*. Not only that, but an almost identical type of cleavage occurs in the northern Taxodiaceae, except *Sequoia*, which shows a derivative of the type. Conditions in the Pinaceae are variable. In some genera there is no cleavage, in others the basal type of cleavage is found. There is here another neat example of a general trend expressed independently but similarly in forms as

systematically distinct as *Torreya*, *Podocarpus*, *Taiwania* and *Keteleeria*. Now there are other types of cleavage such as those shown by *Sequoia gigantea* and *Biota* and, somewhat resembling these, the *Pinus* type. This latter type is of course the type most commonly described in introductory accounts if cleavage is dealt with at all. It is thus of interest to note that this type of cleavage is not even characteristic of the Pinaceae as, apart from *Pinus*, it is met with in only one other genus, *Tsuga*. If, as has been suggested, the genera of the Pinaceae were segregated into smaller families, it is quite certain that *Pinus* and *Tsuga* would be in separate families. It would then appear more clearly that the cleavage in *Pinus*, far from being typical, must be considered a special evolutionary development, practically unique in conifers and expressed elsewhere, and that independently, only in the rather distant *Tsuga*.

There are of course other evolutionary trends in conifer embryology. These cannot be followed now, but some brief reference must be made to the clearly discernible evolutionary trends in gametophytic phases, trends showing the same general character as those shown in embryogeny, modifications which are the expression of evolutionary tendencies occurring independently, but similarly expressed, in forms of diverse affinity. Recognition of parallelism in structural development in groups of varying degrees of affinity is now of course a commonplace in evolutionary theory. In conifer embryology, however, not only are some of the examples almost as striking as the independent development of Oogamy in so many different algal lines but the whole field is dominated by such parallelism.

In a large and varied group such as the podocarps, unless we visualise an extreme polyphyletism, the living genera can be looked on as surviving derivatives of a stock which varied within narrower limits than these derivative types. The members of this stock, and this is in no sense an introduction of typological concepts, must have had their own characteristic embryological features. It always seems to me that their embryology, perhaps of somewhat coarser types, would too have varied only within relatively narrow limits. There must have been a beginning. It is now possible to make tentative suggestions as to what the form, within limits, of the embryological stages of these earlier plants could have been like and to attempt to analyse the nature of the modifications which affected these stages to produce the known, more varied, types of embryology in the podocarps—in other words, to display the developmental tendencies in gametophytes or proembryos. Many such tendencies, up to fourteen or more, can, it is claimed, be discerned in podocarps. One of the most obvious, for instance, would be the change from a few large long archegonia to several smaller and shorter ones. This latter condition is shown for example in *Eupodocarpus* and the rather specialised *Phaerosphaera*. Here we have marked generic divergence systematically but the gametophytic change is the same in both.

The whole group of the conifers can be approached in the same way, though the picture resulting cannot be expected to be as finished as that shown by the podocarps. We have already noted in general the basal plan of embryogeny characterised by many free mitoses and the lack of cleavage polyembryony. The dropping of one or more of the free mitoses is a simple tendency which for instance readily produced from the basal plan four-celled proembryos in genera as diverse as *Athrotaxis*, *Callitris* and *Torreya*. The appearance of a similar type of cleavage polyembryony in, say, *Taxodium* and *Eupodocarpus* is another example. The gametophytic phases produce phenomena just as widespread and as striking.

There is for instance the dropping of the flotation bladders, which we rather mistakenly call wings, from the pollen in Families in which basally the pollen was provided with them. Thus in the Pinaceae this occurred in the *Larix-Pseudotsuga* group, and almost certainly separately, in *Tsuga*. Likewise in the podocarps it is seen in *Saxe-Gothaea* fully and, partially, in *Phyllocladus* and some species of

Dacrydium. It is probable for the araucarians, and at least likely for the taxads and taxodineans, that their bladderless condition is derivative also.

Again reception of pollen by the ovule by means of some sort of exuded pollination fluid is the common mechanism in most conifers, in *Ginkgo* and the cycads. From what Dr. Florin has told us of Permian and earlier conifers it must have been the primitive mechanism also. The loss, however, of the pollination fluid secretion, the reception of pollen on the cone scales, the hyphal-like growth to the ovular micropile of a long free pollen tube, and the partial or marked extrusion of the nucellus for its reception, are parallel conditions developed in forms as distinct as *Araucaria*, *Tsuga* and *Saxe-Gothaea*.

The absence of the primitive prothallial cells from the pollen grain is a well-known character of taxads, cupressineans and taxodineans. In the podocarps, *Phaerosphaera* shows a similar condition, although the family as a whole is characterised by multiple prothallial cells, this latter feature being also paralleled in the araucarians.

We have too the occurrence of grossly unequal male cells in the pollen tube. Normally the body cell divides to give two equal, or at most slightly unequal, male cells. In some cases however a nuclear division at one edge of the body cell produces one large functioning male cell and a minute non-functioning cell which is little more than an extruded nucleus. This phenomenon appears in forms as diverse as *Sciadopitys* and the taxads in the north and in the podocarps in the south. In view of the occurrence of two equal male cells in *Saxe-Gothaea* and *Microcachrys* it is quite possible that the development of grossly unequal male cells has occurred separately in the principal generic lines in the Podocarpaceae.

Again there is the condition in which the pollen tube grows along the side of the female gametophyte to make contact well down with a group of lateral archegonia. This condition is shown by the callitroids and *Athrotaxis* in the south and by *Sequoia* among northern conifers. There can be no affinity except of the remotest kind between say *Callitris* and either *Athrotaxis* or *Sequoia*. Since too there are no authentic fossil records of northern taxodineans in the southern hemisphere or of *Athrotaxis* in the north there can be little connection even between *Athrotaxis* and *Sequoia* beyond that of sharing in some way in a common taxodinean nature. In other words, these quite peculiar gametophyte arrangements must have developed separately in these lines. It is even more remarkable to note in *Athrotaxis* and *Callitris* or *Actinostrobus* the combination of this lateral pollen tube condition with a specialised four-celled proembryo.

These examples must suffice merely to illustrate the point without any pretence at systematic treatment. There are undoubtedly certain features in embryological development which seem to be linked with a particular systematic constitution such as the early binucleate stage in the embryo cells of podocarps but such linkage is not the case in the examples cited, examples taken at random. These show like occurrences, the expression of similar tendencies, cutting right across the lines of systematic affinity. Although in so many cases the expression of these tendencies seems to be haphazard, this appearance probably arises merely from our ignorance of the nature of development.

At any rate it is now possible, at least in a general way, to get some orderly sequence into conifer embryology. We can visualise what the plan, ranging within limits, of early embryology could have been; we can tabulate many of the evolutionary modifications of that plan, the tendencies to change inherent in it; and we can locate the expression, so often apparently haphazard, of these tendencies in the genera and families. Here however we can only state the proposition and leave to other occasions the fuller presentation.

But it clearly follows, from all that has gone before, that embryological data in conifers can only be applied with great discretion, if at all, in the solution of

problems of systematic relationships. The fact for instance that *Callitris* and *Athrotaxis* both show such specialised features as lateral pollen tubes and four-celled pro-embryos seems to have just no bearing at all on their systematic position relative to one another. In the past, of course, it was not always so. Similarities in quite insignificant features of development have been used, sometimes with little other supporting evidence, in efforts to establish affinities between genera and families. Even as recently as 1939, Buchholz, through one of his students, sought to derive *Epherda* from *Juniperus* on the basis of no more than a small similarity of cell behaviour at one stage in the cleavage polyembryony of the two even although in general the total embryology and cleavage types were otherwise quite different. The abuse of such trivia tended to make a travesty of all comparison.

A broader viewpoint is necessary. Conditions may be different in the angiosperms for which I cannot speak, but in the conifers we seem to discern two overlapping groups of evolutionary tendencies. One is concerned with the systematic evolution—the organisation of the family types, the differentiation of the generic lines, the origin of the species. The other group are those associated with modifications in embryological stages. Whatever the causative factors may be, these embryological trends seem to be largely independent of the systematic trends, to be as it were a second pattern imposed upon the first. It will be necessary in the immediate future to clarify the factual relations between those two groups of tendencies and to indicate more clearly than has yet been done in what cases, and how far, the expression of the embryological tendencies may coincide with present ideas on systematy.

But even if that charting were complete, we would still be faced with the more general theoretical problem—what is the nature of the relationship between these two apparently independent but overlapping groups of evolutionary tendencies—the systematic group and the embryological group? Though we may speak of one pattern as superimposed on another, that of course is only a manner of speaking. The relationship is obviously not that of a climbing plant branching and straggling over another shrub. It must be something much more intimate because clearly in any individual the systematic and embryological features make a unity. I can only confess to not understanding it and must leave it to maturer reflection.

There is too the other, the perennial problem—what causative factors can be related to the expression of these embryological tendencies? It does not seem possible to relate them to concepts of survival selection, and my long contact with these phenomena first weakened and then swept away my early confidence in selection as an important evolutionary factor. The selection concept seems rather to be an assumption, based on little factual evidence, which has been so often and so vigorously promulgated that, like the assumption of a heterosporic origin of the seed, it tends to be taken as something established and widely applicable. Granted that there are cases which, if we shut our minds, can with some plausibility be interpreted in terms of selection. But are not these in some quarters overemphasised to the exclusion of the much vaster fields in which our ignorance is profound and about which we could still say, as Proudhon said more than a century ago, 'that we differ from the ancients only in the richness of the language with which we can adorn the obscurity that surrounds us'? What factual evidence is there, apart from mere expression of opinion, that selection is related to macro-evolution, to the appearance of the great animal phyla such as molluscs and arthropods or our own major plant groups? What evidence have we to help us to explain the phenomena of diversification within these groups? Referring to the intricate complexity of the flowering plants, Professor Good has recently expressed the opinion that little or nothing supports the view that they are the product of any highly competitive or eliminative plan. Does not the same apply to diversification within, say, the desmids or the foliose Jungermanniales or to the infinite variety of the centric diatoms which evolved in

the comparatively uniform environment of the Tertiary oceans? The nature too of ontogenetic development, itself an evolution in the strictest sense of the potentialities in the egg, is still a mystery. How does the cytoplasm of the fusiform cambial initial of the conifer produce a bordered pit? What factors determine that extraordinary phenomenon—the alveolation stage in the conifer ovule? Why in the conifer proembryo does the *S* tier elongate? Even Macaulay's schoolboy can glibly talk of the adaptedness shown in the cellular organisation of the leaf of *Ammophila*, but what do we know of the factors determining the form and position of the cells? Modern man may point with pride to his achievements in engineering and electronics—to television, electronic computers, supersonic planes and the like. But he cannot even begin to conceive how he could make a simple blade of grass. He obviously fails here because he knows too little of its form and nature. Since we cannot explain the everyday phenomena of ontogenetic development, it seems to me just intellectual presumption to claim to offer a sort of blanket explanation of the global evolution of all animate nature over a thousand million years of geological time.

Let us then relegate the selection concept to its proper place in the scheme of biological thought. Let us keep it as a hypothesis which is plausible enough in certain minor instances but whose validity in so many major issues is so problematic, so lacking in factual evidence, that only the future, perhaps only the very distant future, can teach us what part it has played in the evolution of the animate universe. Efforts to force everything into the procrustean bed of selection, efforts still made in too many quarters, can only stultify our intellectual perception, particularly our intuitive perception, and so may blind us against newer and more informative ideas. We dare not imagine that biological thought has come to its uttermost limit in 1957 somewhat as

> 'Art stopped short
> In the cultivated court
> Of the Empress Josephine.'

We have before us as a cautionary example how the phlogiston theory dominated chemistry for more than a century in spite of the fact that Rey, 150 years before Lavoisier, had claimed that the calx was heavier than the metal and had suggested that the addition had come from the air. I sometimes fear that an over-preoccupation with selection may prevent us keeping open minds and may bog us down in a phlogiston-like morass. It is pertinent to recall what Bacon wrote concerning certain impediments to the advancement of the sciences. 'Men have used to infect their meditations, opinions and doctrines with some conceits which they have most admired and give all things else a tincture according to them, utterly untrue and improper. So have alchemists made a philosophy out of a few experiments of the furnace. So Cicero, when reciting the several opinions of the nature of the soul he found a musician that held the soul was but a harmony, saith pleasantly "*hic ab arte sua non recessit*". But of these conceits Aristotle speaketh seriously and wisely when he saith "*qui respiciunt ad pauca, de facili pronunciant*"—they who contemplate a few things have no difficulty in deciding.'

THE EDUCATION OF THE COUNTRYMAN[1]

ADDRESS BY MR. L. R. MISSEN, C.M.G. M.C.

PRESIDENT OF SECTION L

" When Adam delved and Eve span,
Who was then the gentleman? "

THERE was a time in our history, of which it is well to remind ourselves occasionally, when it could have been said that almost all the inhabitants of Great Britain and Ireland were countrymen, that those who had gathered into communities were generally living in villages and that towns were rare. Today, however, we are constantly warned by unhappy prophets that the time is fast approaching—at any rate in England—when the rural areas will have been urbanised and there will be no countrymen left except those in the National Parks and Reserves.

Some see in the smaller number of people living in the diminishing countryside a real loss to the nation as a whole and express their belief by opposing extensions of town and city boundaries, the planting of satellite townships in the country, the annihilation of good agricultural land, and the education of country children in urban schools. On the other hand, there are those who think that there is no spiritual or moral quality manifest either in the countryman or in the townsmen in which the other is deficient. If there is any difference, said the late Professor A. W. Ashby,[2] the advantage lies with the countryman in patience, endurance and capacity for continued effort, and probably in austereness and sternness (which may have their drawbacks).

Another writer[3] has said:

differences between town and country are not to be measured simply in terms of physical conditions and amenities. To say that the one is characterised by a rush-hour mentality and that the other's outlook is serener and more settled is to be content with a mere description. At bottom the differences are historical. Because it is static, rural society has contrived to remain unimpressed by the advances of a careerist civilisation and to preserve an integrity which that civilisation finds increasingly impaired.

W. S. Morrison, speaking as Minister of Agriculture on October 20, 1937, expressed the view of many when he said:

I think that in the countryside there is something of real value to the national temperament. A man in the town sees food in the shop window and never perhaps thinks how it got there. But in the country we are constantly brought up against realities. Agriculture is a struggle with nature; and though people in the towns use as a metaphor the expression 'As a man sows so shall he reap', in the country that is not a figure of speech. It is a simple fact. I think something in our rural life does contribute to the nation's make-up a great sense of reality, a sense of cause and effect, which it is difficult to acquire when a man is isolated from kindly Mother Earth by layers of asphalt and concrete.

There are others who could be quoted to show that the countryman, because of his history and upbringing, has his own peculiar contribution to make to the life of the country as a whole, the quality of which may be out of all proportion to his numbers and is to be likened to the pinch of salt which flavours the Scotsman's porridge or the Irishman's potatoes.

Let us turn now to the factual description of the situation, which is given by the Committee on Land Utilisation in Rural Areas in the Report of 1942, generally

[1] Address delivered at the Dublin Meeting of the British Association on September 6, 1957.
[2] Life and Leisure Pamphlet No. 2, p. 21.
[3] Kenneth Richmond, *The Rural School*, p. 79.

known as the Scott Report. More than one-third of the population of England and Wales, said the Report, live in the six largest 'conurbations' of London, Birmingham, Manchester, Liverpool, Leeds-Bradford and Tyneside, and over half live in or near the fourteen chief urban cities. The census returns of 1931 showed that over 80 per cent of the population lived in areas classified as 'urban' for local government purposes, and even this high figure needs to be increased in view of the fact that there are many small towns situated in districts which are classified as 'rural'. The census of 1951 showed a total of 8,422,820 people living in areas described as 'rural' for local government purposes in a total population of 43½ millions in England and Wales.[1] If we define a town as any compact grouping of over 1,500 people, the Scott Report suggests, the total population living outside towns is somewhere in the region of six millions. Of this number, probably two to two and a half millions live in the open countryside, that is to say outside villages; and probably three and a half to four millions live in villages.

No simple definition of rural population or rural community is wholly satisfactory or universally applicable for sociological purposes, according to Ashby, and I shall not attempt to be more precise or to trouble you with any more numbers. The pattern of the countryside or rural district is familiar to the majority of people, and I need only say that the countrymen whose education I have in mind are those who live in groups of villages in the highly cultivated areas, but elsewhere in scattered villages and hamlets and in isolated farms, in the fens, on the moors, and in the mountains.

As urban populations increased and developed their own forms of society or community living, they tended to become isolated from the rural communities around them until transport by rail, by cycle and by motor brought more and more townspeople into contact with the countryman. At the present time contact between town and country is probably as close as it has ever been, but it takes a long time to dispel or remove misconceptions, and there are still some held by the townsman about the countryman. Many people think loosely of the rural dweller as the agricultural labourer and as the worst paid of all the industrial workers. Although there were formerly many more men and women in full-time and part-time employment in agriculture than there now are, they have not constituted the whole of the dwellers in the countryside, and with the gradual reduction in the total number of agricultural workers they represent a still smaller percentage of the whole population than they did.

In such clearly defined rural areas as the Isle of Ely and Norfolk, the total number of full-time agricultural workers represents about 16 per cent and 12 per cent respectively of the total population of the counties, whilst in Suffolk it represents 10 per cent, in Oxfordshire 6 per cent, in Wiltshire 5 per cent and in Westmorland 5 per cent. The total number of agricultural workers, full-time and part-time, in England and Wales in 1956 was given by the Ministry of Agriculture as 547,853 of whom the full-time male and female workers numbered:

Male over 21	365,882
Male under 21	72,956
Female	39,831
Full-time	Total 478,669

This is, of course, a large number for any one industry, and it is made very much larger when wives, children and dependents are added. The variety and number of the other countrymen must not, however, be overlooked, and I make no excuse for mentioning a few amongst many in the full list. There are shopkeepers, innkeepers, policemen, postmen, clergy, teachers, doctors, chemists, Ministry of

[1] For 1953–4 figures for U.K., see G. D. H. Cole in *The Post-War Condition of Britain*.

Agriculture staff, commercial agricultural staff, builders, blacksmiths, mechanics and garage men, transport drivers, small manufacturers, dairymen, canners, maltsters, millers, local goverment clerical and technical workers, water and sewerage engineers, roadmen, electricity board employees, nurses, midwives and health visitors and so on, for many of whom, as we shall see later, a wide variety of social, recreational and educational provision is made.

The most serious misconception is that the countryman is the mental inferior of the townsman, that there is a higher incidence of mental deficiency amongst countrymen, and that their children are 'dullards'. During the war and in the years that have followed, the nation has become increasingly conscious of the importance of agriculture and aware of the skill and the knowledge required to produce food, so that the farmer and his men are now regarded as possessing considerable ability and technical knowledge. The townsman is less inclined to call the farmworker a 'country bumpkin' or 'yokel'; but the misconception about the children persists and to some extent affects the recruitment of teachers to village schools.

In the field of mental deficiency my own county of Suffolk has often been held to ridicule and has traditionally been called 'Silly Suffolk', but this is now acknowledged to have been due to a mistake. The adjective applied originally to the county was 'selig'—an Anglo-Saxon word meaning 'blessed' or 'holy'—and was so applied because of the very large number of churches which the county contains. It is by no means inferior to any other, either mentally or physically, as the records of its men and women show.

For the past hundred years or more there has been a continual movement of people from our countryside into the towns and at times to all parts of the Commonwealth and the Colonies. (Orwin[1] reported a 48 per cent decrease in agricultural workers between 1870 and 1931.) Doubtless many very good men and women were amongst them, but I know of no proof that migration is selective and leaves an inferior residuum. I do not agree with the writer, who says:

> Those who showed any signs of ability have left the country for some more lucrative jobs in the towns: the remainder have gone on performing the same mean tasks with the regularity and the inevitability of the seasons themselves. It follows automatically that their children must be slow-witted and of the earth, earthy.

Indeed, I quote against him Professor Stapledon, who says

> To hold to the country despite everything and for sheer love of the country implies strength of character rather than the reverse. I think it is just as legitimate, because just as well substantiated by the facts as we know them, to give prominence to this selective influence as to lack of initiative.

The enquiry into mental deficiency by Dr. Lewis for the Mental Deficiency Committee 1929, contained this statement, after a careful survey of the facts from the areas investigated: 'It is only the impetuous reader who will conclude from our data that rural inhabitants as a group are generally inferior in mental endowments to the inhabitants of urban areas'. (Part 4, p. 72). After his most careful examination of all the printed evidence, Dr. D. J. Davies,[2] of University College, Aberystwyth, says:

> We may conclude that the existing evidence with regard to particular types of mental deficiency suggests that migration may have had some influence in increasing deficiency ratios in the rural areas, but that the major question, as to whether migration has operated selectively so as to lower the mental quality of the rural population in comparison with its own past or with towns, must be held to be open. The verdict for the time being is 'not proven'.

[1] *Problems of the Countryside* (1945), p. 7.
[2] *Physical and Mental Condition of the Rural Population*, 1929.

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[1] For 1953–4 figures for U.K., see G. D. H. Cole in *The Post-War Condition of Britain.*

Agriculture staff, commercial agricultural staff, builders, blacksmiths, mechanics and garage men, transport drivers, small manufacturers, dairymen, canners, maltsters, millers, local goverment clerical and technical workers, water and sewerage engineers, roadmen, electricity board employees, nurses, midwives and health visitors and so on, for many of whom, as we shall see later, a wide variety of social, recreational and educational provision is made.

The most serious misconception is that the countryman is the mental inferior of the townsman, that there is a higher incidence of mental deficiency amongst countrymen, and that their children are 'dullards'. During the war and in the years that have followed, the nation has become increasingly conscious of the importance of agriculture and aware of the skill and the knowledge required to produce food, so that the farmer and his men are now regarded as possessing considerable ability and technical knowledge. The townsman is less inclined to call the farmworker a 'country bumpkin' or 'yokel'; but the misconception about the children persists and to some extent affects the recruitment of teachers to village schools.

In the field of mental deficiency my own county of Suffolk has often been held to ridicule and has traditionally been called 'Silly Suffolk', but this is now acknowledged to have been due to a mistake. The adjective applied originally to the county was 'selig'—an Anglo-Saxon word meaning 'blessed' or 'holy'—and was so applied because of the very large number of churches which the county contains. It is by no means inferior to any other, either mentally or physically, as the records of its men and women show.

For the past hundred years or more there has been a continual movement of people from our countryside into the towns and at times to all parts of the Commonwealth and the Colonies. (Orwin[1] reported a 48 per cent decrease in agricultural workers between 1870 and 1931.) Doubtless many very good men and women were amongst them, but I know of no proof that migration is selective and leaves an inferior residuum. I do not agree with the writer, who says:

> Those who showed any signs of ability have left the country for some more lucrative jobs in the towns: the remainder have gone on performing the same mean tasks with the regularity and the inevitability of the seasons themselves. It follows automatically that their children must be slow-witted and of the earth, earthy.

Indeed, I quote against him Professor Stapledon, who says

> To hold to the country despite everything and for sheer love of the country implies strength of character rather than the reverse. I think it is just as legitimate, because just as well substantiated by the facts as we know them, to give prominence to this selective influence as to lack of initiative.

The enquiry into mental deficiency by Dr. Lewis for the Mental Deficiency Committee 1929, contained this statement, after a careful survey of the facts from the areas investigated: 'It is only the impetuous reader who will conclude from our data that rural inhabitants as a group are generally inferior in mental endowments to the inhabitants of urban areas'. (Part 4, p. 72). After his most careful examination of all the printed evidence, Dr. D. J. Davies,[2] of University College, Aberystwyth, says:

> We may conclude that the existing evidence with regard to particular types of mental deficiency suggests that migration may have had some influence in increasing deficiency ratios in the rural areas, but that the major question, as to whether migration has operated selectively so as to lower the mental quality of the rural population in comparison with its own past or with towns, must be held to be open. The verdict for the time being is 'not proven'.

[1] *Problems of the Countryside* (1945), p. 7.
[2] *Physical and Mental Condition of the Rural Population*, 1929.

Let me turn now to the children. Country children, it is sometimes suggested, are on the whole duller and less capable of intellectual development than town children. Though the weight of evidence is against it, says Kenneth Richmond, a certain sly and private conviction of the country child's dullness remains in the popular mind.

The late Sir Godfrey Thomson, of Moray House, Edinburgh, some five or six years ago directed an investigation into the intelligence quotients of children aged twelve, living in rural areas in England, and East Suffolk was included in his survey. In his report he compared the results with those obtained from town children of the same age. Taking a number of municipal boroughs, he found the mean I.Q. of boys to be 101·373 and of girls 102·492. In the rural districts (including East Suffolk) the figure for boys was 98·951 and that for girls was 100·991. In other words, the town boy was two and a half points above the rural boy and the town girl was one and a half points above the country girl. Even if these tests are accepted as infallible, it must be admitted that there is very little in the differences. The tests applied were of course devices for doing a practical job in sorting children out and were not instruments devised to provide information about the structure of the children's minds. 'There are no tests' L. M. Terman[1] says, 'which are absolutely sure tests of intelligence. All are influenced to a greater or a lesser degree also by training and environment.'

Another investigation was made in the Scottish Mental Survey.[2] Commenting on the results, the Report says:

> We have remarked on the difficulty that must always be experienced in generalising as to urban and rural intelligence where identity of racial, socio-economic and geographical conditions can seldom be present. Certainly Scottish rural children would not appear to be on the average inferior in intelligence to Scottish urban children.

How then do we account for the differences which undoubtedly exist between the rural school child and other schoolchildren?

In the first place, the tempo of life, the rate at which things happen, is slower in the countryside than in the town, and it is against this background, quieter, more leisurely—with time 'to stand and stare'—and moving, as it were, with the seasons, that you must picture the rural schoolchildren. I am told by those who have taught in town and country that these children do not accept what they are told so readily as town children do, nor do they ask for so much explanation as the town child does, but they get to the objective in time, and having accepted it, they keep it. M. K. Ashby, in the *Country School* (1929), says:

> Children in agricultural districts are undoubtedly slower in comprehension of certain groups of facts and ideas than those in the towns. At the same time teachers of experience maintain that they are more thorough, and that in the long run their mental equipment is as good as that of many town children. They have not the same stimulus to quick thinking; but they are not so superficial and have greater power of perseverance and endurance.
> Maybe he is not so quick on the uptake, [says another writer,[3]] but then everything in his nature and upbringing warns him against looking for quick returns. Like a field of corn, he cannot be hurried. His performance may not be brilliant but it has a steady-going reliability which gets there in the end.

The point was well illustrated by one of my staff who is concerned with their physical training.

> If I was asked, [he said,] to give an 'on the spot' demonstration of a physical training lesson and had the choice of a town class or a country class, both entirely unknown to me,

[1] *The Measurement of Intelligence*, p. 135.
[2] *The Intelligence of a Representative Group of Scottish Children*, 1933.
[3] Kenneth Richmond, *The Rural School*, p. 151.

I would choose the town class without any hesitation. But if I was asked to give a prepared demonstration after a few days of preparation I would take either with equal confidence.

I have similar evidence from the teachers of academic subjects. Self-expression in the written word does not come easily to the country child, but that he can write well in his own style is shown in his essays and in his school magazines. In speech he is not, on the whole, as articulate or as rapid as the urban child, largely because of the home influence. Landworkers work a great deal alone and tend to taciturnity, so that conversation in some homes is dull and the children are not encouraged to talk much. Play-acting and oral work bring them out at school but they will not be forthcoming with a stranger whose motives they do not comprehend. If, however, you ask a schoolchild, as I do on all possible occasions, for direction in finding a house or village, you will get brief but clear instruction from a child obviously glad to be of help to you. It is a mistake to suppose that rural children know the names and ways of all the birds and the names of all the wildflowers. They should perhaps know more than the children living in the middle of a great city, but they need to be taught like all other children. They have such opportunity to study nature—'to go and out see her riches and partake in her rejoicing with heaven and earth', as Milton put it—that the teaching is easier than it would be in the city; but, although the rural child lives where it is possible to find seventy different plants in a short length of hedge or thirty different living creatures in a pond, he is just as surprised and interested as the urban child would be when he does this with a teacher.

I have no time to pursue this theme any further, but I hope I have done something to dispel, wherever it may linger, the idea that in dealing with rural men and women we are dealing with a section of the population which is inferior to the men and women of the urban areas and cities 'You who live fourteen miles from a market town have become a kind of holy vegetable', said Sidney Smith. And Hazlitt said: 'The country is a kind of healthy grave.' These statements are not true of England and Wales today, no matter what their significance may have been in the past.

Let us now turn to an appraisal of some of the instruments of education for the countryman, and first for the boy or girl in the countryside. The latest report of the Ministry of Education covering the year 1955 has a table on page 130 which shows the distribution of schools in England and Wales by size. The eye is caught immediately by the figures for the small schools. Those with a roll of up to 25 pupils, which are single-teacher schools, total 1,224 in the rural areas and only 56 in the urban areas. The all-age schools in the 1,224 number 103. Similarly, schools with a roll of 26 up to 50, which are two-teacher schools, total 753 in rural areas and 237 in urban. In 1947, the first year after the war for which there is a Report, the up-to-25 schools totalled 1,937 and the 25-to-50 schools were 3,292. It will be seen that some three thousand little schools have closed in ten years. Some could not be kept open, others could not be kept up!

The problem of the very small school is mainly a problem for the rural area, and this is also the situation in a world-wide setting from South-East Asia through India and Africa to the West Indies, in the Middle East, and doubtless in Russia and the Far East. With you here in the Republic of Ireland, there are over eight hundred single-teacher schools, whilst in New Zealand there are slightly less. In the United States of America there are about 70,000. The output of books and reports in the last twenty years shows that the problem has excited world-wide interest, and UNESCO, in one of their Abstracts (Vol. VI, No. 5), summarises recent literature upon it from France, Germany, Italy, Australia, New Zealand, Latin America, Austria and Great Britain.

The most carefully documented report which I have examined is E. W. Parkyn's *The Consolidation of Rural Schools*, published by the New Zealand Council for Educational Research. Whereas in England and Wales very little was attempted in

the way of reorganisation of schools either in urban or in rural areas until after the publication of the Hadow Report on the Education of the Adolescent, in November 1926, in New Zealand (where there were 1,500 single-teacher schools twenty years ago) the process of 'consolidation', as it is named, had been attempted much earlier and included not only the gathering together of children of secondary school age into larger units but the amalgamation of small isolated schools, all of which involved the use of transport and the provision of amenities for children spending the whole day in a school. In New Zealand the Wanganui Education Board reported in 1889 that 'with a view to economy as well as increased efficiency in the teaching staff' it had attempted with success the amalgamation of the smaller schools. The Otago Education Board arranged for the transport of pupils, and by 1912 'it was strongly recommended' that consolidation should proceed. This was the first time the term 'consolidation' was officially used. Evidence of the effect of this operation was placed before the Hadow Committee in 1925. In 1946 the New Zealand Council for Educational Research sponsored a comprehensive study of the educational and social effects of consolidation. G. W. Parkyn, the research officer, reports with diligence and is objective. The findings are most valuable in measuring the problem of small schools. He says:

(i) the large schools have the advantage in classification;
(ii) the small schools on the whole have done better work in the basic skills than all but the largest town schools;
(iii) the large schools have the advantage in specialist staff; and music, art, literature and cultural studies generally are very uneven in the small school;
(iv) a wider circle of friendship is open to the child in the large school, but children in the small school learn to be responsible for the younger ones and to be looked after by the older ones in a way that is characteristic of family life;
(v) the older children in the small school are unable to develop their deeper social interests;
(vi) teacher and child in the small school get to know one another more intimately than elsewhere, and this understanding gives security to the child in the early years of life;
(vii) the play in the small school suits the social and emotional growth of little children but is restricted for older ones;
(viii) close relations between parents and teacher are fostered in the small school.

I think these findings echo the experience of teachers in England and Wales and are worth while repeating in connection with the current amalgamation of small schools and the organisation of secondary schools for children over eleven years of age. You will decide for yourselves which way the scale is tilted by the arguments. I will tell you how, in fact, the situation has altered in England and Wales.

First the single-teacher and two-teacher schools. Owing to the deplorable state of many of the school buildings the number of them has been reduced through closure in the following circumstances:

1. The number of children of school age has fallen so low (say below ten) as to make the cost of maintenance unjustifiable and transport to a neighbouring school has been arranged.
2. The building has deteriorated to such an extent that it has to be vacated.
3. No teacher would apply for the vacant post or posts because of the physical conditions outside, e.g. isolation in a remote area—working all alone in good health or in bad, inadequate lodging or housing, etc., and inside, e.g. the state of the building, heating and sanitation, etc. At Easter this year the President of the National Union of Teachers at their Margate Conference called attention to the large number of black-listed schools in use. They were black-listed by the Board of Education;

they were black-listed in 1926; and there are 564 of them, some in urban and some in rural areas. This is a disgrace to the educational system of England and Wales.

Of these three conditions the third in particular is now speeding the closure of single-teacher schools. The continued shortage of women teachers and their wider choice of posts, together with the local education authority's need to deploy staff to the best advantage of the children; and the growth and availability of suitable transport together with the provision of milk and midday meals, make the closure of almost all single-teacher schools a certainty.

The critics' point of view about these schools has been put by Canon E. F. Braley, reported in *Education* (August 21, 1942) to have said:

> I am satisfied that it is beyond the powers of any man or woman properly to educate fifteen to twenty-five children of different ages in a single schoolroom. We are all familiar with the spectacle of the teacher in the village school passing from one group to another, and trying to direct the education of all at the same time. We are also familiar with the continual distractions and lack of quietness which accompany such an attempt. From time to time, for what it is worth, I have found that the I.Q.s of village children are at least as high and often higher than those of children in urban areas. It is indisputable that village children are most seriously handicapped compared with the children attending large schools. If the state is determined, as part of its post-war reconstruction policy, to give each child equality of educational opportunity, there can be no doubt that one- or two-teacher schools, as they exist today, must disappear from our educational system.

It is a good thing for country children generally that they can in increasing number be taught in two-teacher or larger schools; but I cannot pass from this topic of the single-teacher school without paying a special tribute to those teachers who have spent their lives or large parts of their lives in them. To teach about twenty children whose ages range from five to eleven, or even fifteen, and whose abilities and interests stubbornly resist uniformity, is excitingly difficult. To be successful in this kind of school (and many teachers do succeed) you must, in my opinion, be 'called' to the work. Those who are good are very, very good; but there were never enough of them, and now there never will be. In the lives of their pupils and in the lives of their pupils' children and even grandchildren, they have their reward.

The countryside schools have been well served in their histories by the boys and girls who have gone thence to college to qualify as teachers and have come back to teach in familiar surroundings. But the countryside should not depend, any more than a city, upon meeting its own need entirely from amongst its own people. There should be no barrier between the town-bred or the country-bred teacher in the field of teaching and free movement for all of them is welcomed. The countryside will be the loser, nay, it is now the loser, through the sad condition of many of its schools causing the country-bred teacher to teach in the towns and preventing any appreciable reciprocity through the flow of town-bred teachers to the country. Although the blame must lie largely in the physical conditions which deter them from rural teaching, it must also be said that insufficient interest in the past has been shown by training colleges and departments, though there are now courses of training which are of special benefit to any one proposing to teach in the country.

I must now say a little about the greatest change in education in the countryside in this century, the establishment of secondary modern schools following the reorganisation of schools which was set in motion by the Hadow Report of 1926. In the original area schools or village colleges, and now in the modern schools, all of which provide for children of secondary school age—eleven and upwards—the children from groups of neighbouring villages have been brought together by bicycle and bus to work and to play in larger communities. This change did not come too soon; indeed, it has not come yet to all the countryside. In spite of circulars

from the Ministry of Education and the reiterated statements of policy by the Government, the provision of secondary education in the countryside lags behind that of the towns and there are still many modern schools to be built to replace the all-standard provision in which boys and girls, to the profound dissatisfaction of their parents, continue to be taught. The significance of what this means in the countryside has never been fully realised in Whitehall or Curzon Street. Once the hand had been put to the plough the furrows should have been turned in steady sequence! As it is now, two generations of a family may have passed through reorganised secondary education in one village whilst a few miles away their cousins have 'made do' with old-fashioned premises and inadequate physical and science facilities or, more likely, none at all. How long indeed will the patience of the countryman last? Could there be any more blatant example of inequality of opportunity? If the building of the remaining modern schools is not in fact speeded up and completed by 1961–2, three generations of one village will have had secondary education while their neighbours still have none.

I would mention two fundamental results of the new secondary school education, clear enough where this has been most successful with the pupils. Without it the countryman would have been seriously handicapped in his new world of mechanisation and in the application of science to the land. It has given him in the leisured and well-ordered development of a good modern school an opportunity to grow at his own pace and to reach standards in academic and in practical subjects at a later stage, it may be, than his town cousin, but at the same level, and to make himself or herself fit to go on to stages of further education connected with earning a livelihood, or acquiring culture, which he could never have done before. This is all of immense benefit to the countryman in making his contribution to the national good.

Without belittling in any way the work of the rural grammar schools, or their contribution, very often at high level, to the life of the countryside, I must say that the new modern school offers most exciting prospects for the future, and as I see it, is already providing a new source of well-trained and competent young men and women to reinforce the front line of the advance in which new developments are being harnessed to the service of man.

The rural grammar school provides for the twenty per cent or thereabouts of country children who are found in the eleven year old age group each year to be suited by aptitude and ability for courses leading to 'O' and 'A' levels in the General Certificate of Education. The geographical distribution of these schools is largely traditional and many of them have long and honourable histories in the small country towns where they are often housed in ancient and inadequate premises. Their pupils have in the past been handicapped by the long distances they have had to travel and the shortened hours of attendance which prevented full participation in games and out-of-school activities. More generous boarding and lodging arrangements and improved transport have enabled these boys and girls to enjoy a fuller and richer school life, and rural parents have not been left behind others in seizing the advantages of a longer school life for their children. The consequent increase in the numbers has enlarged some of the smallest grammar schools into units for which more generous staffing can be made and in all of them there is a most encouraging and valuable increase in sixth form numbers. Bearing in mind the difficulty of providing specialists in all the subjects and the higher degree of adaptability required in the staffs of small grammar schools, it must be said that these schools have made a great contribution to life not only in the countryside but nationally. They have maintained high standards and have provided the doctors, the solicitors, the clergy and other professional men and women for country life as well as many for the towns and cities, and for the fields of foreign service.

One of the most remarkable changes in the countryside in the past twenty years is the growth in the number of tractors to be seen in the fields and the fall in the

number of horses, so much so that there are now some farmers who retain a couple of ploughing teams solely as reminders to the younger generation of how the work used to be done. Between 1942 and 1954 the number of horses used for agricultural purposes on farms in England and Wales decreased from 484,782 to 152,899, a fall of 331,883, averaging about 30,000 a year! And in the same period the following increases have occurred in machines:

All tractors—from 101,505 to 'well over 400,000'.
Milking machines—from 23,856 to 120,000
Combine harvesters—from 150 to 31,000.
Grain dryers—from a few to 4,793
Tractor mould-board ploughs—from 97,174 to 360,000.
(The figures are taken from *Farming Figures*, published by the Association of Agriculture.)

In the villages there is now a pressing demand for men and boys with a sound knowledge of the internal-combustion engine, of electricity and petrol, of chemical manures, insecticides and spraying plant. The farmer and his workers have had to adapt themselves to very rapid changes.

How has the educational structure met their needs? First, I suppose, through the Agricultural Advisory Service and through demonstrations and talks arranged by the Ministry of Agriculture. Next, through the evening classes of the local education authority, and, in some small measure, through day-release classes which are growing side by side with the Agricultural Apprentices Scheme. By means of this scheme, selected boys from town and country are apprenticed for a fixed period to selected farmers and can work, through part-time day and evening classes, for a National Certificate. Up to April 1956 over six hundred apprentices had been placed in agriculture through this scheme, but in some areas the farmers appear to have been reluctant to join in and development is slow. It is encouraging, however, that there are reported to be very many boys anxious to become apprentices. The number of Farm Institutes, increased from seventeen to thirty-six as a result of the Loveday Report which was published ten years ago, appears to be adequate for the full-time technical training of the countryman and there are 2,065 places for men and women, not all of which are filled. The Minister of Agriculture recently appointed a Departmental Committee under Earl de la Warr 'to review the provision of Further Education for Agriculture made by Local Education Authorities with particular reference to the Farm Institutes and to make recommendations'.

This Committee will doubtless take into account the fact that the hours of work and the physical demands made upon the individual worker have been very much lowered in the last few years so that it is now possible that a wider system of part-time agricultural education might successfully supplement the Farm Institute system in day and evening classes; and that a financial recognition of the gaining of the National Certificate or the completion of a one-year full-time course would put technical education in agriculture on a parallel with technical education in industry. Agricultural education is not, of course, the only kind of further education offered to the countryman. Apart from those who leave home to reside in the universities, or institutions of further education and technical training, there is a large number who pursue, after leaving school, the study of commercial and industrial subjects, preparing themselves by evening study for examinations leading to national diplomas and certificates. They do this at greater cost in time and effort than do many townsmen and, as the classes are held for the most part in wintertime, at some exposure to the weather. It is a tribute to their 'capacity for continued effort' that so many succeed; but in this field too a great deal remains to be done to make the opportunities of further education under the Act of 1944 a reality.

In the field of leisure-time occupation the countryman is in some ways better off

than the townsman. He is less inclined, and has less opportunity, to assemble in large numbers to watch football matches and dog and dirt-track racing; or to sit in cinemas on two or three evenings a week. He is a good reader, as the system of county libraries and the record of the borrowing of books clearly shows. The younger generation has a choice of youth clubs and voluntary organisations, some of which may be nearby, while others are cycling or bus distances away, but there are still not enough of these. Education committees have in varying degree covered their areas with evening institutes which offer a wide variety of educational, cultural and recreational activity, and there are drama and music groups, as well as produce, beekeeping, pig and goat and similar associations. Young Farmers' Clubs number 70,000 members and cover all the counties. National Farmers' and Agricultural Workers' Unions have very extensive membership. The National Federation of Women's Institutes has sixty-one County Federations and includes the Channel Islands and the Isle of Man, with 456,949 members in 8,306 Institutes. The Mothers' Union and other organisations attached to the religious denominations have a large membership. With all these activities there is movement and visits are paid and meetings arranged by districts, deaneries, counties and dioceses, so that there is frequent coming and going between villages and life does not stand still. The bicycle and the bus carry many further afield and marriages are contracted between people living further apart than used to be the case, so that a wider variety of surnames has come to the villages.

'The countryman's social feelings, attitudes, views and standards', says Ashby, 'have been adjusted primarily to a relatively narrow social environment, to the family, to the kindred, to the village.' There is no doubt now that the schools are bringing up generations of boys and girls who look out and still further out as their experiences are enriched by classroom, playing-field, school excursion and home and foreign travel. At the same time those who were born too early to have the advantage of modern secondary education are striving, not too late, in ever-increasing numbers, to enter into the wider experiences of life.

By and large I suppose the countryman in his leisure time is more often doing things than watching things being done; often, however, he is not doing anything but merely 'passing the time'. As F. G. Thomas says in *The Changing Village*, 'he ruminates', and when he is like that, one may say of him, as was said of the younger Scipio; 'He was never less alone than when he was by himself.' The Welsh Department of Education, in *Education in Rural Wales*, states that:

> It can hardly be denied that the men and women who live most closely to the great rhythms of the natural world possess a greater integration within themselves than is common among those who live in cities. They are most serene and inwardly poised, less susceptible to fluctuations of spiritual moods and depression of material resources, because their roots are deeper, their primary necessities are assured and their characters have been steadily formed by the influences of nature and the unhurried circuit of changeful seasonal occupations.

When I was making an advisory visit to the Minister of Education in Iraq, I went to see a Training College for Teachers in Rural Schools. It was situated in the hills and remote from city life, for the Iraquis regarded the city as in some way a vortex which swallowed what was intended for the country; and to counteract this in part, they paid their rural teachers a higher salary rate. After the midday meal I paid a courtesy visit to the *Kaimaquam* or head of the village community and was hospitably entertained. We sat talking all the afternoon and most of the evening as we looked out from his house over a fertile and peaceful valley, remote from the modern world. At last we rose to say good-bye, and taking my hands in his, he said: 'It has been a great pleasure to me to sit and talk of the serious things of life. May Allah protect you on your way and may you always have time for consideration.'

THE ROLE OF AGRICULTURE IN THE NATIONAL ECONOMY[1]

ADDRESS BY SIR JAMES TURNER

PRESIDENT OF SECTION M

IT is a signal honour for me to give this Presidential address to you today. I speak as a farmer, one whose life has been absorbed completely in the land, both in working it, and in thinking about its problems and its needs. Of course, as some of you may know, I am given frequent opportunities to think at large about the place of agriculture in the national economy—in particular, round about February each year when I am invited by Agricultural Ministers to consider, in company with a group of people, the industry's economic condition and prospects.

It is a salutary and rewarding experience for a farmer thus to relate his own world to the context of the larger world around him. I want to do this here for a special reason. Today, more than ever perhaps in British history, our future as a nation depends vitally upon a continuing surge of technical progress, with all the economic and social adjustments that implies. My theme will be, therefore, that a full measure of technical progress in the future will depend very much upon the agricultural industry being able to count upon the right basis of political and economic conditions.

A glance at the past will help us to view the present period of development in perspective.

The first great period of economic and technical development in agriculture as we approach modern times occurred roughly in the period of the Industrial Revolution, that is, during the years between about 1750 and the end of the Napoleonic Wars. New industries gave people employment in the towns and the country's population rose very considerably. Gregory King, who in 1688 made a rough guess at the occupational distribution of the total population in England and Wales, estimated at $5\frac{1}{2}$ million people, suggested that freeholders, farmers and their families comprised some $1\frac{3}{4}$ million, and that, in addition, there were over $1\frac{1}{4}$ million 'cottagers and paupers', many of whom had some rights in land. By the beginning of the nineteenth century, however, Britain was gradually developing her industrial face, and there was the problem of feeding the increasing urban population with a diminished agricultural labour force. Thus we find that in 1831 the total population of England and Wales was just under 14 millions, of whom only about a million were engaged on the land.

The traditional methods associated with the old open-field system were no longer adequate. In the great enclosure movement of George III's reign, $5\frac{1}{2}$ million acres were fenced off and consolidated for more economic production.

The growing urban demand for livestock products stimulated a change in emphasis from arable to stock farming. This was the period of that great innovator Robert Bakewell, who, by his principle of inbreeding for special characteristics, paved the way for the later supremacy of British livestock on the farms of the world. However, one should perhaps mention here that the earlier work of Jethro Tull and 'Turnip' Townsend in developing the root crop for winter feed enabled the efforts of the breeders to be fully exploited.

We are told, for example, that the average weight of cattle and sheep sold at Smithfield market doubled in the period 1710–95. All this was, of course, associated

[1] Address delivered at the Dublin Meeting of the British Association on Thursday, September 5, 1957.

with the better integration of arable and stock farming which in turn was only feasible because of the disappearance of the open fields.

It is worth noting also that there was a very close connexion between the vigorous growth of our agriculture in this period and the development of our modern system for providing bank credit.

With the collapse of prices after the Napoleonic Wars and the generally depressed state of the industry, there was a slowing-down of technical development. The 1850's, however, saw the beginning of a new phase of development in our agriculture. International trade received a fillip from the gold discoveries in California. Again, the Crimean War and the American Civil War stimulated an expansion in our home grain production.

So, one hundred years ago began the so-called golden age of British agriculture, when high farming became general, with the virtual completion of the enclosures and the wider application of new techniques like the four-course Norfolk rotational system of husbandry introduced some time before by such men as Coke. Following the development of new methods in the 1830's and 1840's by people like Josiah Parks and Thomas Scragg, large areas, especially on heavy clay lands, were drained more effectively than before. The age of rapid mechanical change also saw improvements to farm implements. New varieties of ploughs, the iron harrow, corn and seed drills, horse-hoes, hay-makers and horse-rakes were increasingly used. The reaper, invented in 1826, and threshing and winnowing machines already invented in the eighteenth century, did not come into general use until after 1853. It was during this period that the formation of pedigree herds of cattle and sheep became general. Most important of all, however, the direct application of science to agriculture was seen in the field of agricultural chemistry. Liebig in Germany, for example, and later the workers at the new experimental station at Rothamsted, studied how to replace in the soil the chemical nutrients required by each crop.

This picture of scientific progress lasted, however, only until the 1870's. From then until the end of the era of free trade, the technical apparatus of farming became progressively out of date as a further period of depression overtook the industry.

Advocates of free trade argue that the absence of trade barriers was the means by which Britain achieved industrial pre-eminence in the nineteenth century; the same people are prepared to find excuses for the concomitant decline of British agriculture. I think, however, it would be truer to contend that we achieved our dominating economic position as a country during the nineteenth century simply because we reaped all the advantages of being first in the industrial field. We had the manufactures to sell and other nations were eager to buy them, only too willing to sell us in return their agricultural produce on exceptionally favourable terms. However, we bought these advantages at the expense of the decline of our rural population and a complete separation between our urban and our rural economy.

The attitude of the time is admirably summarised by Trevelyan, who writes:

> 'The belief in Free Trade as the secret of our vast prosperity, the unwillingness to interfere with the world commerce on which our power and wealth seemed to stand secure, the predominance of the towns over the country in numbers and still more in intellectual and political leadership, the memories of the "hungry forties" when the Corn Laws had made bread dear for the poor—all these circumstances prevented any effort to save the rural way of life.'

Other European countries, however, who still had a peasantry and valued their rural populations as an important stabilising element in the social structure, sought the protection of tariffs against the influx of cheap food from America, Australia and New Zealand. This tradition of protectionism has, of course, survived right up to our own times.

One school of thought has argued that hard times should ultimately force farmers

to institute improvements so as to cut their costs in order to be in a better position to recover markets lost to the competition of cheaper supplies—and these cheaper supplies, we must remember, were often the result of the prodigal exploitation of virgin resources. The marginal cost of producing food in Britain would have fallen to a level no higher than the marginal cost of buying it from abroad. That was the theory. In practice what happened was that farmers, surrounded by economic depression, neither possessed nor could borrow the resources needed to improve their permanent and other equipment and to apply improved techniques.

In the past eighteen years the United Kingdom has seen, perhaps more clearly than at any other time in its history, that it is unwise to allow the free operation of the market alone to determine the level of economic activity in the farming industry. In that period, when agriculture had guaranteed markets for most of its products, investment rose to a level which, as we know from pre-war experience, was far higher than would otherwise have been the case. Since a higher level of investment is usually associated with increased productivity, it follows that had the industry in this period been fully exposed to the waywardness of the market, investment would have been checked, and its productivity would not have climbed to the present high level.

Our economic survival in the United Kingdom has depended upon extracting, from our own land, more of the food we eat—consistent, that is, with keeping the land in good heart. To ensure a higher level of domestic production, it has been necessary for the State, representing the consumers in this country, to enter into a contract with producers through the system of price guarantees and assured markets. This has been done because, as a country, we have consciously decided that we cannot allow future levels of agricultural production to be wholly determined by the free movement of market prices which, for agricultural products, are inherently vulnerable to relatively small changes in supply and demand. Although the extent of economic planning in the overall sense may be a matter for debate, nobody would seriously dispute that the structure established by the Agriculture Act of 1947 has been responsible for the expansion of our production to a level some 60 per cent above pre-war, so enabling us to satisfy half the food needs of the community. It has been interesting to note, in fact, that in the Parliamentary debates on the Agriculture Bill, 1957, not a single voice was raised against the principle of support for agriculture; the differences of opinion arose chiefly on the *extent* to which the State should intervene, rather than on the principle of intervention itself. The general acceptance of State support for the industry by the two main parties in Britain is, of course, a major political development of our time as far as agriculture is concerned.

The third great age of technical and scientific development in British agriculture—the present one—has thus gone hand-in-hand with this transformation of the industry's economic and political background. I am convinced that future historians will regard the technical progress during the present era as surpassing even that of the Agrarian Revolution or the Golden Age. In our times, we have witnessed the application of mechanical power to the land. The internal-combustion engine and electricity have radically augmented the power of the individual agricultural worker. I do not think I need to catalogue the specific developments as they are sufficiently well known. The fact must be stressed, however, that without consolidation of the industry's financial basis, farmers would simply not have been in a position to take advantage of the new discoveries.

I cannot find that there is any serious body of opinion which would now argue against the principle of State support for British agriculture. The principle is of world-wide application today though the method certainly takes different forms. Indeed, different governments in the United Kingdom have interpreted the principle differently. For example, the present British Government believes that

producers of a particular review commodity should, in the aggregate, be helped by means of standard prices and deficiency payments and that, as individuals, they should sell their commodity on a free market. In other words, the consumer should be the final arbiter as to the price in the market, and he should also be allowed free access to imported as well as home-produced supplies of food.

Lest it be thought that the policy of trade liberalisation belongs to the present British Government alone, it is only fair to say that both in the OEEC Convention and in the GATT, the post-war Labour Government entered into commitments that provided the momentum for the progressive removal of tariffs and quotas. From 1951 onwards, Conservative Governments have carried out essentially the same policy, though it should be pointed out that special arrangements were made to permit the revision of tariffs on horticultural products which do not come under the price support arrangements affecting the basic agricultural commodities. This revision made in 1953 and 1954 was an attempt to restore some of the effectiveness of the pre-war tariff. With the fall in money values, the specific pre-war duties had lost almost all their effectiveness. But even since that revision, further developments have created a need for yet another revision to make these duties fully effective at the present time. To my mind, it is a pity that in committing us as a country to these international agreements the Government of the day did not recognise, and make adequate provision for, the special considerations affecting production and trade in agricultural products which might merit some form of import regulation. The application of general rules that might be suitable for removing barriers to our international trade in manufactured products could conceivably do great harm to our agriculture because of the industry's special problem.

Let me explain this point. Productive resources in agriculture cannot easily be switched from one commodity to another. Moreover, the production cycle is generally much longer than in most manufacturing industries; not only is it less adjustable than in manufacturing industry, but it is very much controlled by seasonal influences. Any sudden increase in, say, foreign beef shipments can have a disproportionate effect on prices. We had a classic example of this in the spring and summer of 1956. Market prices for fat cattle were, on average, some 40*s*. per live cwt. *less* than in the previous year. We had another example in the case of bacon this spring when because of a marginal change in the supply/demand ratio, and aggravated by shipments of Danish bacon on to our market, the wholesale price of English No. 1 selection fell, in a matter of ten weeks, from 328*s*. to 247*s*. per cwt., a drop of 25 per cent. If producers were expected to plan output on the basis of these sharp and sudden swings in the market, they would obviously play for safety by investing less, restricting output, producing it at higher unit cost, and thereby increasing in the long run the cost of living for consumers.

As it is, the free play of the market means that from time to time deficiency payments may suddenly increase or just as suddenly fall in bridging the gap between producer and consumer prices. Some form of import regulation, by helping to stabilise consumer prices, would also assist in reducing the cost to the Exchequer.

The gain to producers from stable markets is fairly obvious. To consumers and taxpayers, the benefits are complementary, accruing from the avoidance either of sharp changes in the cost of living, with their consequent impact on wages and production costs, or of sudden variation in Exchequer liability. It is really a little unreasonable for the customer to expect to have it both ways, that is, to rely upon the home producer for a major share of his needs while expecting to pay low prices—often below production costs when the market is glutted—and at the same time to criticise the heavy subsidies consequent upon this fall in price. I am sure that the Government should give more attention to the relation between market returns and the cost of Exchequer support.

The need for regulating imports is strengthened by the fact that under current

systems of supporting the cereals and livestock sections of the industry, it is not the individual producer to whom a price is guaranteed but producers as a whole. Thus it happens that two producers, marketing similar grades of livestock products on the same day, receive quite different prices in the markets and yet both receive the identical guarantee payment.

It is, for example, absurd that a producer who may have spent up to five years in planning the production of a choice beef animal should suffer from the impact on his local market of the arrival of a ship carrying chilled beef on that particular day. While the guarantee structure provides a buffer for the industry as a whole, it leaves the individual producer open to this kind of haphazard contingency, which ought to be avoidable in any well-regulated scheme of things. I should perhaps add that the impact of such vacillations in the market for the end-product—that is, the finished animal—also injures the producer of the store animal. To the extent that the finisher is led to be cautious in his buying, the interests of the store producer are seriously prejudiced.

However, in discussing import policy we have to bear in mind continually the influence of the GATT and the OEEC Convention. No doubt they are of great value to the U.K. as an industrial exporting country, but for the British farmer their value is contingent upon a general awareness of the special problems of the agricultural sector of the economy. It is a slow job persuading Governments to this point of view, but there is no doubt that we must persevere in the task.

The extent to which trends in supply are to be allowed to influence changes in the price guarantees for the basic agricultural commodities at the Annual Price Review is now restricted, of course, by the new long-term assurances provided for under the Agriculture Bill, 1957,[1] and implemented for the first time administratively at the 1957 Review. By limiting the extent to which the overall value of the guarantees to the industry can be cut, and by narrowing the margin for reductions in the guarantees for individual commodities, the structure of agricultural support established by the 1947 Act is accordingly strengthened. In these measures, the Government has aimed at a policy that will put the price guarantees on to a long-term basis and so provide that stable foundation which is essential if we are to continue to progress towards higher and higher levels of efficiency.

I have already pointed out that today there is no longer any substantial support for the view that prices on the free market should be the sole determinant of the volume of production in agriculture. This shift of opinion has placed on all of us, however, the obligation to devise new criteria, since it must obviously be in everybody's interest to ensure that the food that comes from our own resources is produced at the lowest unit cost. In saying this, I do not wish in any way to give the influence of the market anything less than its proper importance. Obviously, we must have regard to the long-term trend of consumer demand for individual commodities and due account must be taken of these trends in determining the price guarantees at the annual reviews. I do claim, however, that in view of the long-term character of agricultural production, and its vulnerability to climatic and other factors, the individual producer should be protected from the full impact of the necessarily unpredictable changes in the free market. This, of course, is one of the purposes of deficiency payments which bridge the gap between the price guarantees and market prices at any particular time. However, the need for support, in the long run, can only be reduced without prejudice to the producer's financial position if two factors are working in the right direction: firstly, if his unit production costs are going down; secondly, if, through improved marketing, his sales receipts are going up. I want now to look at these aspects in rather more detail.

Let us be clear at the outset that the industry's ability to lower unit costs does not depend solely upon its own efforts but also upon two extraneous factors. The first

[1] *N.B.* The Bill is expected to be on the Statute Book by September 1957.

of these is the Government's ability to halt inflation; and the second is the efficiency of those industries which supply agriculture with goods and services—these are valued currently at some £800 million a year. For the moment, however, let us consider agriculture's ability to manage its resources better. For this purpose, let us assume that the prices of goods and services supplied to agriculture remain constant—though I indeed hope that these prices will go down! The technical problem then becomes one of obtaining more output per unit of input. It is a pity that nobody has yet been able to calculate accurately what the agricultural industry as a whole has actually been achieving by way of increased technical productivity in recent years. We have so far failed to reach agreement with the Government on the right method of calculating increased efficiency and we have to make do with the rather general statements on this matter which are to be found in Annual Price Review White Papers. I am aware that there is a difference between technical productivity on the one hand and economic productivity on the other. By technical productivity I mean getting the maximum physical output from such resources as are to be found on the farm. By economic productivity I mean the combination of these resources in such a way as to maximise *profit* as distinct from output. Obviously the technical optimum and the economic optimum do not necessarily coincide. However, I think that a satisfactory means of measuring technical productivity alone would be of great value to everyone concerned with discussions affecting the economic, political, and technical future of the industry. For example, at the Annual Price Review such a yardstick would provide us with reliable guidance as to the extent to which producers could absorb cost increases without loss of net income at any given level of production.

I think most of you will be aware that the economists of the Ministry of Agriculture prepare detailed calculations annually of the output and expenditure of the agricultural industry. From these figures it is possible to make a rough estimate of the net aggregate income of agriculture. This basic data is also used to measure the change in the volume of production. If, however, the Ministry of Agriculture could be persuaded to attempt a calculation of the net change in the volume of agricultural inputs, I feel that we might be nearer to filling the statistical gap to which I have just referred—nearer, that is, to obtaining a yardstick of technical productivity. I suggest that this is the type of question on which research might be carried out by the agricultural economists of our universities. I am sure that the results of such research would be of more than purely academic interest.

Whatever may have been done in the past to improve efficiency, an even greater challenge now lies before us. With the new long-term assurances, and now that the arrangements for capital grants under the Farm Improvement Scheme are coming into operation, the challenge is to find new and ever-better ways of putting our productive resources to the most economic use. Perhaps I may mention here some of the directions in which I think our efforts should be concentrated in the years ahead.

First of all, there is no doubt that grass, the United Kingdom's largest and most important crop, offers great scope for rapid development in yield, utilisation, and conservation. There have been great strides, during the last twenty years, in the production of improved strains of grasses and clovers. There is a better appreciation of the intensive use of fertilisers. New techniques have been developed not only for the better production and management of grass, but also for its more efficient utilisation by means of rotational grazing and by improved methods of conserving it for winter feed.

In all these ways has our knowledge increased, placing in our hands the power—given proper access to capital—to raise our productive efficiency higher and higher. There is no doubt, also, that continuing progress in scientific investigation will push still further the frontiers of our knowledge, and so increase still further our power to make the earth produce.

Turning from a particular application of agricultural science to the general application of better farming methods, I think there can be no doubt as to the contribution that work study will make to agriculture in the coming years. As our friends in manufacturing industry are still discovering, wherever work is being performed, there is a place for work study.

I should like, by way of example, to mention briefly two recent cases of applying work study on farms. The first relates to silage-making on a farm where previously the job required two permanent employees and two casual workers. With work study methods, plus the spending of eighty pounds on capital equipment, the field work, transport, loading and filling of the silo were accomplished in half the time, and there was no need for the two casual labourers.

The second case was a work study carried out in a piggery. A new procedure was worked out, involving minor modifications to the building. As a result, the saving in time added up to no less than five working weeks in the year.

I think we have reached the stage when work study knowledge and advice should be made available to farmers on a basis similar to that of the National Agricultural Advisory Service. And I think that every District Officer of the Advisory Service should receive work study training. This would enable him to appreciate fully the contribution that work study can make to farm management in general.

Apart from the suitability of field processes for work study application, there is no doubt that much of the work in and around the buildings of the farmstead, particularly that concerned with the management of livestock, offers tremendous scope for re-design as the result of work study. Many of our existing farm buildings were designed for quite different conditions of labour and methods of work. Now that the industry, through the medium of the Farm Improvement Scheme, will be spending quite large sums on new permanent equipment, we need a new interest in farm-building design among our architects, builders and industrial designers. Clearly, there is a fruitful and stimulating line of activity here for the architectural profession and the various skills related to it. The money which will be spent by farmers and by the State on buildings in the coming years will not be used to the best advantage unless there is a thorough understanding, in the light of work study principles, of the functional requirements of farm buildings. To encourage architects and others in this line of endeavour, the National Farmers' Union of England and Wales has recently established a Research Fellowship in the Design of Farm Buildings, which is to be administered by the Nuffield Foundation. It is our sincere hope that the existence of this Fellowship, and the original work which will result from it, will lead our architects and designers to take an active part, with the farmer himself, in what I might call the 'streamlining' of both his equipment and his work.

I should like to turn now to a different aspect of agricultural productivity. Efforts to reduce our unit costs of production would, of course, be assisted materially if the prices of the goods and services which we buy in from the ancillary industries could themselves be reduced. Apart from wage rates, which are fixed by an independent statutory board, and the tax element (including import duties) on fuel and other requisites, there is no other direct intervention by the State nowadays in the prices of the industry's factors of production. The structure of the businesses engaged in supplying commodities for use in agriculture betrays a variety almost as considerable as does agriculture itself. At one extreme we have large integrated organisations possessing what is virtually a monopoly in the supply of the requisite; at the other, we have the small country merchant selling a large variety of goods in his general service store. Obviously all these firms have a direct interest in the financial stability of the agricultural industry. In effect, they enjoy the benefits of the price support and production grant arrangements indirectly through the maintenance of demand for their products.

From time to time, the question is asked whether they pass on the full benefits of

their increased efficiency to the agricultural producers on whom their livelihood depends. One cannot answer this question because there is little published information about changes in their costs of production, profitability, etc. I do, of course, appreciate that in so far as imported raw materials constitute an important part of the total cost of any particular requisite, movements in world markets are bound to have some influence on our costs. At the same time, such movements should not be used as an excuse for automatically passing on to the farmer the full amount of any increased costs. I readily acknowledge the excellent record of a large number of important suppliers in this respect, but I think it is a pity that, while farmers are always under pressure on the score of efficiency, so little is ever said about the efficiency of the ancillary industries. They have the benefit of a firmly based home market, and they should share with us the obligation to cut at least their operational costs to the minimum and so allow the consumer, through the medium of the farmer, to share in that benefit.

Earlier on I spoke about the need for stability in the general level of the market as a prerequisite for raising productive efficiency. I must now turn to the contribution which producers themselves can make towards more orderly marketing and towards reducing Exchequer liability by obtaining better returns from improved product promotion and more streamlined distribution. Agricultural marketing has been developed in this country largely with the aid of the statutory boards set up under the Agricultural Marketing Acts, 1931 to 1949. Statutory trading boards are now in being for milk, wool, potatoes, hops and eggs, and they are concerned with the disposal of approximately 40 per cent of our total agricultural output of some £1,400 million, compared with about 30 per cent five years ago. In addition, there is a non-trading scheme for tomatoes and cucumbers. However, organised marketing has gone further than the statutory schemes. The Fatstock Marketing Corporation, launched by the three Farmers' Unions in 1954, is now playing a most significant role in improving the first-hand distribution of meat; last year the value of produce it dealt with was about £110 million. Again the agricultural Co-operative Marketing Societies in the United Kingdom handled over £50 million worth of farm products last year compared with £42 million five years ago. While these figures cannot be added together, because of some double counting, they do serve to indicate the substantial improvement that has taken place in producer marketing in recent years.

Producers have supported statutory and co-operative schemes of marketing because in all of them the aim is to improve producer returns by the elimination of waste in distribution costs—in other words, to ensure that more of every pound the consumer spends at the retail end comes back to the producer. Before the war, for example, the Milk Marketing Boards made substantial reductions in the cost of transporting milk from the farm to dairy and creamery. The Milk Boards, by their promotional campaigns, have also helped to increase liquid consumption of milk per head from the pre-war figure of three and a quarter pints per week to the present five pints—an increase of over 50 per cent. In its relatively short period of operation since 1951, the Wool Board has had considerable success in keeping marketing costs down by negotiating charges centrally with merchants, insurance companies, etc. Moreover, through better marketing, it has secured a higher average price for the British wool clip than would otherwise have been the case—thereby reducing the need for any Exchequer support.

To take yet another example, the Fatstock Marketing Corporation, in taking over the central procurement system for bacon pigs previously operated by the Government, prevented what would almost certainly have been chaotic conditions when pig marketing was decontrolled in 1954. The Corporation has made itself responsible for contracting with the bacon factories and has then set itself to secure from producers the pigs needed to meet these contracts. The task has been extraordinarily

difficult because to obtain all its requirements the Corporation has had to operate and compete in a free market no matter what the strength of the alternative demands for pig-meat happened to be. However, there is no doubt that, without the Corporation's central procurement system, conditions on the bacon market would have shown much sharper fluctuations, both in volume of supplies coming forward, and in price. I could give you many other instances of the achievements of the producer-marketing movement in Britain. There is no doubt that it ranks, side by side with the guarantee system, as one of the two most revolutionary agricultural developments of our generation.

However, these developments in producer-marketing at one end, and the gradual return of selective food consciousness on the part of the consumer at the other, have not been accompanied by similar progress in the distribution system. It is in the sphere of really energetic emphasis on the presentation, pre-packaging, and self-service distribution of branded British foodstuffs, effectively advertised and carrying a continuing guarantee of quality, that early and dramatic advances can be achieved.

May I now attempt to sum up. As I see it, the need for further economic and technical development in British agriculture is today as great, if not greater, than at any other time in its history. It will be necessary for us to do much more than merely progress at the *same* rate as our competitors overseas. We can only improve our relative position if our rate of progress here exceeds what is being done abroad, for only in this way shall we achieve parity of *real* costs between our products and those of our competitors. I emphasise the word 'real' because I would not accept for one moment that a simple comparison between our prices and those of our overseas suppliers is in fact a valid one. If we want a true comparison of costs of production we must look behind the screen constituted by the support arrangements in operation in many of the countries from which we import food. However, progress in productive efficiency is going on all over the world, and our task is to see to it that our unit costs go down faster than the prices of those who would seek to market their products competitively in the United Kingdom, and as I have shown, this does not depend on the agricultural industry alone.

I have already indicated some of the means of achieving this aim. We must find better ways of translating knowledge from the laboratory to the farm. This means an improved and strengthened advisory service; it also means the further development of such techniques as work study. Financially it requires a system of guarantees that do not fluctuate wildly from year to year in response to what are in many cases purely transient movements in the market.

But price-guarantee systems are not the whole story. No business is normally expected to find the whole of its investment needs out of income; part of the capital for development can be supplied only from external sources. The organisation of agriculture largely in one-man businesses makes it difficult for producers to raise capital from outside. This raises the whole question of agricultural credit. If development is to proceed in the future at full vigour it is not just a case of obtaining more seasonal finance from the banks or merchants. There must be full credit facilities for long-term and medium-term improvements. There is an overwhelming need to strengthen the existing arrangements for providing agricultural capital and credit, especially if farmers are to be in a position to make the best use of the Farm Improvement Scheme and the various production grants.

What we really need is a combined operation in the economic, financial, technical, and scientific fields if we are to succeed in transforming the structural basis of the industry during the next generation, on a scale comparable to the Agrarian Revolution of the eighteenth century. After all, we must remember that, despite the great strides British agriculture has taken in the last eighteen years, we still have to make up for a whole seventy years of depression and neglect which have left their mark on the industry—particularly in its permanent equipment. In the years since 1939,

in responding to the national need for more home-grown food, the industry has been forced to concentrate on short-term improvements such as mechanisation, the use of fertilisers, and the building-up of T.T. herds. Frankly, we must face the fact that there are thousands of farms in the United Kingdom in need of improvement and redesign in their permanent equipment, and that this is going to cost a lot of money. However, from the national as well as the individual producer standpoint, it will be money well spent if it results, as it will, in lowering our unit costs of production.

This is the thought, therefore, that I wish to leave in your minds today. As an industry we are in a period of re-equipment and re-tooling which bids fair to be every bit as memorable to future historians as the eighteenth-century period of development is to us. With a stable support structure, and a proper flow of credit, there will be unlimited scope for the application of new knowledge. New knowledge —new skills—new ways of doing old tasks!

It is a salutary thought that agriculture is in fact man's most ancient industry, his very first attempt so to organise the natural resources around him as to provide a dependable supply of food to eat and clothes to put on his body. But just as agriculture itself rests upon a never-ending process of renewal, so we farmers must ensure that old knowledge is continually overlaid and renewed with new science, and with new wisdom. The British Association itself is based upon the implicit belief that the range of man's potential wisdom is endless, and that each frontier of knowledge which he reaches will merely open up a view of yet another vast territory of the mind for him to conquer. Holding fast to that belief myself, I feel abundant confidence that there will be no lack of new knowledge flowing into agriculture, and no shortage of vigour and enterprise among our farmers to add that new knowledge to the accumulated wisdom of their ancient art—the husbandry of the good earth.